PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCVII.

PART II.

LONDON,

SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.
MDCCXCVII.

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XI. *On the Action of Nitre upon Gold and Platina.* By Smithson Tennant, *Esq. F. R. S.*

Read March 23, 1797.

GOLD, which cannot be calcined by exposure to heat and air, has been also considered as incapable of being affected by nitre. But in the course of some experiments on the diamond, an account of which has been communicated to this Society, I observed, that when nitre was heated in a tube of gold, and the diamond was not in sufficient quantity to supply the alkali of the nitre with fixed air, a part of the gold was dissolved. From this observation I was induced to examine more particularly the action of nitre upon gold, as well as to inquire whether it would produce any effect upon silver and platina.

With this intention I put some thin pieces of gold into the tube together with nitre, and exposed them to a strong red heat for two or three hours. After the tube was taken from the fire the part of the nitre which remained, consisting of caustic alkali, and of nitre partially decomposed, weighed

140 grains; and 60 grains of the gold were found to have been dissolved. Upon the addition of water about 50 grains of the gold were precipitated, in the form of a black powder. The gold which was thus precipitated was principally in its metallic state, the greater portion of it being insoluble in marine acid. The remaining gold, about 10 grains in weight, communicated to the alkaline solution, in which it was retained, a light yellow colour. By dropping into this solution diluted vitriolic or nitrous acid, it became at first of a deeper yellow, but if viewed by the transmitted light, it soon appeared green, and afterwards blue. This alteration of the colour from yellow to blue arises from the gradual precipitation of the gold in its metallic form, which by the transmitted light is of a blue colour. Though the gold is precipitated from this solution in its metallic form, yet there seems to be no doubt that while it remains dissolved it is entirely in the state of calx. Its precipitation in the metallic state is occasioned by the nitre contained in the solution, which having lost part of its oxygen by heat, appears to be capable of attracting it from the calx of gold; for I found that if the calx of gold is dissolved by being boiled in caustic alkali, and a sufficient quantity of nitre which has lost some of its air by heat is mixed with it, the gold is precipitated by an acid in its metallic state.*

* As the precipitation of gold in its metallic form, by nitre which has lost some of its oxygen has not, I believe, been noticed, it may not be improper to mention some of those facts relating to it which seem most entitled to attention. Nitre which has been heated some time precipitates gold in its metallic state from a solution in aqua regia, if it is diluted with water. If a solution of gold in nitrous acid is dropped into pure water, the calx of gold is separated, which is of a yellow colour; but if the water contains a very small proportion of nitre which has lost some of its air by heat (as one grain in six ounces), the gold is deprived of its oxygen, and becomes blue. The

Having found that nitre would dissolve gold, I tried whether it would produce any effect upon platina.

It has been formerly observed that the grains of platina, in the impure state in which it is originally found, might, by being long heated in a crucible with nitre, be reduced to powder. LEWIS, from his own experiments and those of MARGRAAF, thought that the iron only which is contained in the grains of platina was corroded by the nitre. But by heating nitre with some thin pieces of pure platina in a cup of the same metal, I found that the platina was easily dissolved, the cup being much corroded, and the thin pieces entirely destroyed. By dissolving the saline matter in water, the greater part of the platina was precipitated in the form of a brown powder. This powder, which was entirely soluble in marine acid, consisted of the calx of platina, combined with a portion of alkali, which could not be separated by being boiled in water. The platina which was retained by the alkaline solution communicated to it a brown-yellow colour. By adding an acid to it a precipitate was formed, which consisted of the calx of platina, of alkali, and of the acid which was employed.

Silver, I found to be a little corroded by nitre. But as its action upon that metal was very inconsiderable, it did not appear to be deserving of a more particular examination.

alkali of the nitre does not assist in producing this effect. Nitrous acid alone, which does not contain its full proportion of oxygen, occasions the same precipitation, unless it is very strong; and if a mixture of such strong nitrous acid, and of a solution of gold in nitrous acid, is dropped into water, the gold is deprived of its oxygen, and is precipitated of a blue colour. Two causes contribute to produce this effect upon the addition of water. The adhesion of the calx of gold to nitrous acid is by that means weakened, and the oxygen is attracted more strongly to the imperfect nitrous acid, in consequence of their attraction for water when they are united.

XII. *Experiments to determine the Force of fired Gunpowder.* *By* Benjamin *Count of* Rumford, *F. R. S. M. R. I. A.*

Read May 4, 1797.

No human invention of which we have any authentic records, except, perhaps, the art of printing, has produced such important changes in civil society as the invention of gunpowder. Yet, notwithstanding the uses to which this wonderful agent is applied are so extensive, and though its operations are as surprising as they are important, it seems not to have hitherto been examined with that care and perseverance which it deserves. The explosion of gunpowder is certainly one of the most surprising phænomena we are acquainted with, and I am persuaded it would much oftener have been the subject of the investigations of speculative philosophers, as well as of professional men, in this age of inquiry, were it not for the danger attending the experiments: but the force of gunpowder is so great, and its effects so sudden and so terrible, that, notwithstanding all the precautions possible, there is ever a considerable degree of danger attending the management of it, as I have more than once found to my cost.

Several eminent philosophers and mathematicians, it is true, have, from time to time, employed their attention upon this curious subject; and the modern improvements in chemistry have given us a considerable insight into the cause, and the

nature of the explosion which takes place in the inflammation of gunpowder; and the nature and properties of the elastic fluids generated in its combustion. But the great desideratum, the real measure of the initial expansive force of inflamed gunpowder, so far from being known, has hitherto been rather guessed at than determined; and no argument can be more convincing to show our total ignorance upon that subject, than the difference in the opinions of the greatest mathematicians of the age, who have undertaken its investigation.

The ingenious Mr. ROBINS, who made a great number of very curious experiments upon gunpowder, and who, I believe, has done more towards perfecting the art of gunnery than any other individual, concluded, as the result of all his inquiries and computations, that the force of the elastic fluid generated in the combustion of gunpowder is 1000 times greater than the mean pressure of the atmosphere. But the celebrated mathematician DANIEL BERNOUILLI determines its force to be not less than 10,000 times that pressure, or ten times greater than Mr. ROBINS made it.

Struck with this great difference in the results of the computations of these two able mathematicians, as well as with the subject itself, which appeared to me to be both curious and important, I many years ago set about making experiments upon gunpowder, with a view principally of determining the point in question, namely, *its initial expansive force* when fired; and I have ever since, occasionally, from time to time, as I have found leisure and convenient opportunities, continued these inquiries.

In a paper printed in the year 1781, in the LXXI. Volume of the Philosophical Transactions, I gave an account of an

experiment (No. 92.) by which it appeared that, calculating even upon Mr. ROBINS's own principles, the force of gunpowder, instead of being 1000 times, must at least be 1308 times greater than the mean pressure of the atmosphere. However, not only that experiment, but many others, mentioned in the same paper, had given me abundant reason to conclude that the principles assumed by Mr. ROBINS, in his treatise upon gunnery, were erroneous; and I saw no possibility of ever being able to determine the initial force of gunpowder by the methods he had proposed, and which I had till then followed in my experiments. Unwilling to abandon a pursuit which had already cost me much pains, I came to a resolution to strike out a new road, and to endeavour to ascertain the force of gunpowder by *actual measurement*, in a direct and decisive experiment.

I shall not here give a detail of the numerous difficulties and disappointments I met with in the course of these dangerous pursuits; it will be sufficient briefly to mention the plan of operations I formed, in order to obtain the end I proposed, and to give a cursory view of the train of unsuccessful experiments by which I was at length led to the discovery of the truly astonishing force of gunpowder;—a force at least *fifty thousand* times greater than the mean pressure of the atmosphere!

My first attempts were to fire gunpowder in a confined space, thinking, that when I had accomplished this, I should find means, without much difficulty, to measure its elastic force. To this end, I caused a short gun-barrel to be made, of the best wrought iron, and of uncommon strength; the diameter of its bore was $\frac{1}{4}$ of an inch, its length 5 inches, and the thickness of the metal was equal to the diameter of the bore, so that its external diameter was $2\frac{1}{4}$ inches. It was closed at both

ends, by two long screws, like the breech-pin of a musket; each of which entered 5 inches into the bore, leaving only a vacuity of 1 inch in length for the charge. The powder was introduced into this cavity by taking out one of the screws, or breech-pins; which being afterwards screwed into its place again, and both ends of the barrel closed up, fire was communicated to the powder by a very narrow vent, made in the axis of one of the breech-pins for that purpose. The chamber, which was 1 inch in length, and $\frac{3}{4}$ of an inch in diameter, being about half filled with powder, I expected that when the powder should be fired, the generated elastic fluid being obliged to issue out at so small an opening as the vent, which was no more than $\frac{1}{10}$ of an inch in diameter, instead of giving a smart report, would come out with something like a hissing noise; and I intended, in a future experiment, to confine the generated elastic fluid entirely, by adding a valve to the vent, as I had done in some of my experiments mentioned in my paper published in the LXXI. Volume of the Philosophical Transactions. But when I set fire to the charge (which I took the precaution to do by means of a train), instead of a hissing noise, I was surprised by a very sharp and a very loud report; and, upon examining the barrel, I found the vent augmented to at least four times its former dimensions, and both the screws loosened.

Finding, by the result of this experiment, that I had to do with an agent much more troublesome to manage than I had imagined, I redoubled my precautions. As the barrel was not essentially injured, its ends were now closed up by two new screws, which were firmly fixed in their places by solder, and a new vent was opened in the barrel itself. As both ends of

the barrel were now closed up, it was necessary, in order to introduce the powder into the chamber, to make it pass through the vent, or to convey it through some other aperture made for that purpose. The method I employed was as follows: a hole being made in the barrel, about $\frac{1}{10}$ of an inch in diameter, a plug of steel was screwed into this hole; and it was in the centre or axis of the plug that the vent was made. To introduce the powder into the chamber the plug was taken away. The vent was made conical, its largest diameter being inwards, or opening into the chamber; and a conical pin, of hardened steel, was fitted into it; which pin was intended to serve as a valve for closing up the vent, as soon as the powder in the chamber should be inflamed. To give a passage to the fire through the vent in entering the chamber, this pin was pushed a little inwards, so as to leave a small vacuity between its surface and the concave surface of the bore of the vent. But notwithstanding all possible care was taken in the construction of this instrument, to render it perfect in all its parts, the experiment was as unsuccessful as the former: upon firing the powder in the chamber, (though it did not fill more than half its cavity), the generated elastic fluid not only forced its way through the vent, notwithstanding the valve (which appeared not to have had time to close), but it issued with such an astonishing velocity from this small aperture, that instead of coming out with a hissing noise, it gave a report nearly as sharp and as loud as a common musket. Upon examining the vent-plug and the pin, they were both found to be much corroded and damaged; though I had taken the precaution to harden them both before I made the experiment.

I afterwards repeated the experiment with a simple vent,

made very narrow, and lined with gold to prevent its being corroded by the acid vapour generated in the combustion of the gunpowder; but this vent was found, upon trial, to be as little able to withstand the amazing force of the inflamed gunpowder as the others. It was so much, and so irregularly corroded, by the explosion in the first experiment, as to be rendered quite unserviceable; and what is still more extraordinary, the barrel itself, notwithstanding its amazing strength, was blown out into the form of a cask; and though it was cracked, it was not burst quite asunder, nor did it appear that any of the generated elastic fluid had escaped through the crack. The barrel, in the state it was found after this experiment, is still in my possession.

These unsuccessful attempts, and many others of a similar nature, of which it is not necessary to give a particular account, as they all tended to shew that the force of fired gunpowder is in fact much greater than has generally been imagined, instead of discouraging me from pursuing these inquiries, served only to excite my curiosity still more, and to stimulate me to further exertions.

These researches did not by any means appear to me as being merely speculative; on the contrary, I considered the determination of the real force of the elastic fluid generated in the combustion of gunpowder as a matter of great importance.

The use of gunpowder is become so extensive, that very important mechanical improvements can hardly fail to result from any new discoveries relative to its force, and the law of its action. Most of the computations that have hitherto been made relative to the action of gunpowder, have been founded upon the supposition that the elasticity of the generated fluid is as its

density; but if this supposition should prove false, all those computations, with all the practical rules founded on them, must necessarily be erroneous; and the influence of these errors must be as extensive as the uses to which gunpowder is applied.

Having found by experience how difficult it is to confine the elastic vapour generated in the combustion of gunpowder, when the smallest opening is left by which any part of it can escape, it occurred to me, that I might perhaps succeed better by closing up the powder entirely, in such a manner as to leave no opening whatever, by which it could communicate with the external air; and by setting the powder on fire, by causing the heat employed for that purpose to pass through the solid substance of the iron barrel used for confining it. In order to make this experiment, I caused a new barrel to be constructed for that purpose: its length was 3.45 inches, and the diameter of its bore $\frac{1}{10}$ of an inch; its ends were closed up by two screws, each one inch in length, which were firmly and immoveably fixed in their places by solder; a vacuity being left between them in the barrel 1.45 inch in length, which constituted the chamber of the piece; and whose capacity was nearly $\frac{4}{10}$ of a cubic inch. An hole, 0.37 of an inch in diameter, being bored through both sides of the barrel, through the centre of the chamber, and at right angles to its axis, two tubes of iron, 0.37 of an inch in diameter, the diameter of whose bore was $\frac{1}{10}$ of an inch, were firmly fixed in this hole with solder, in such a manner that while their internal openings were exactly opposite to each other, and on opposite sides of the chamber, the axes of their bores were in the same right line. The shortest of these tubes, which pro-

jected 1.3 inch beyond the external surface of the barrel, was closed at its projecting end, or rather it was not bored quite through its whole length, $\frac{1}{75}$ of an inch of solid metal being left at its end, which was rounded off in the form of a blunt point. The longer tube, which projected 2.7 inches beyond the surface of the barrel on the other side, and which served for introducing the powder into the chamber, was open; but it could occasionally be closed by a strong screw, furnished with a collar of oiled leather, which was provided for that purpose. The method of making use of this instrument was as follows. The barrel being laid down, or held, in a horizontal position, with the long tube upwards, the charge, which was of the very best fine-grained glazed powder, was poured through this tube into the chamber. In doing this, care must be taken that the cavity of the short tube be completely filled with powder, and this can best be done by pouring in only a small quantity of powder at first, and then, by striking the barrel with a hammer, cause the powder to descend into the short tube. When, by introducing a priming-wire through the long tube, it is found that the short tube is full, it ought to be gently pressed together, or rammed down, by means of the priming-wire, in order to prevent its falling back into the chamber upon moving the barrel out of the horizontal position. The short tube being properly filled, the rest of the charge may be introduced into the chamber, and the end of the long tube closed up by its screw.

More effectually to prevent the elastic fluid generated in the combustion of the charge from finding a passage to escape by this opening, after the charge was introduced into the

chamber, the cavity of the long tube was filled up with cold tallow, and the screw that closed up its end (which was $\frac{1}{2}$ an inch long, and but a little more than $\frac{1}{10}$ of an inch in diameter) was pressed down against its leather collar with the utmost force. The manner of setting fire to the charge was as follows: a block of wrought iron, about $1\frac{1}{2}$ inch square, with a hole in it, capable of receiving nearly the whole of that part of the *short tube* which projects beyond the barrel, being heated red hot, the end of the short tube was introduced into this hole, where it was suffered to remain till the heat, having penetrated the tube, set fire to the powder it contained, and the inflammation was *from thence* communicated to the powder in the chamber.

The result of this experiment fully answered my expectations. The generated elastic fluid was so completely confined that no part of it could make its escape. The report of the explosion was so very feeble, as hardly to be audible: indeed it did not by any means deserve the name of a report, and certainly could not have been heard at the distance of twenty paces; it resembled the noise which is occasioned by the breaking of a very small glass tube.

I imagined at first that the powder had not all taken fire, but the heat of the barrel soon convinced me that the explosion must have taken place, and after waiting near half an hour, upon loosening the screw which closed the end of the long vent tube, the confined elastic vapours rushed out with considerable force, and with a noise like that attending the discharge of an air-gun. The quantity of powder made use of in the experiment was indeed very small, not amounting to

more than $\frac{1}{8}$ part of what the chamber was capable of containing; but having so often had my machinery destroyed in experiments of this sort, I began now to be more cautious.

Having found means to confine the elastic vapour generated in the combustion of gunpowder, my next attempts were to measure its force; but here again I met with new and almost insurmountable difficulties. To measure the expansive force of the vapour, it was necessary to bring it to act upon a moveable body of known dimensions, and whose resistance to the efforts of the fluid could be accurately determined; but this was found to be extremely difficult. I attempted it in various ways, but without success. I caused a hole to be bored in the axis of one of the screws, or breech-pins, which closed up the ends of the barrel just described, and fitting a piston of hardened steel into this hole (which was $\frac{3}{10}$ of an inch in diameter), and causing the end of the piston which projected beyond the end of the barrel to act upon a heavy weight, suspended as a pendulum to a long iron rod, I hoped, by knowing the velocity acquired by the weight, from the length of the arc described by it in its ascent, to be able to calculate the pressure of the elastic vapour by which it was put in motion; but this contrivance was not found to answer, nor did any of the various alterations and improvements I afterwards made in the machinery render the results of the experiment at all satisfactory. It was not only found almost impossible to prevent the escape of the elastic fluid by the sides of the piston, but the results of apparently similar experiments were so very different, and so uncertain, that I was often totally at a loss to account for these extraordinary variations. I was however at length led to suspect, what I afterwards found abundant

reason to conclude was the real cause of these variations, and of all the principal difficulties which attended the ascertaining the force of fired gunpowder by the methods I had hitherto pursued.

It has generally been believed, after Mr. ROBINS, that the force of fired gunpowder consists in the action of a permanently elastic fluid, similar in many respects to common atmospheric air; which being generated from the powder in combustion, in great abundance, and being moreover in a very compressed state, and its elasticity being much augmented by the heat (which is likewise generated in the combustion), it escapes with great violence, by every avenue; and produces that loud report, and all those terrible effects, which attend the explosion of gunpowder.

But though this theory is very plausible, and seems upon a cursory view of the subject to account in a satisfactory manner for all the phænomena, yet a more careful examination will shew it to be defective. There is no doubt but the permanently elastic fluids, generated in the combustion of gunpowder, *assist* in producing those effects which result from its explosion; but it will be found, I believe, upon ascertaining the real expansive force of fired gunpowder, that this cause, alone, is quite inadequate to the effects actually produced; and that, therefore, the agency of some other power must necessarily be called in to its assistance.

Mr. ROBINS has shewn, that if all the permanently elastic fluid generated in the combustion of gunpowder be compressed in the space originally occupied by the powder, and if this fluid so compressed be supposed to be heated to the intense heat of red-hot iron, its elastic force *in that case* will be 1000

times greater than the mean pressure of the atmosphere; and this, according to his theory, is the real measure of the force of gunpowder, *fired in a cavity which it exactly fills.*

But what will become of this theory, and of all the suppositions upon which it is founded, if I shall be able to prove, as I hope to do in the most satisfactory manner, that the force of fired gunpowder, instead of being 1000 times, is at least 50,000 greater than the mean pressure of the atmosphere?

For my part, I know of no way of accounting for this enormous force, but by supposing it to arise principally from the elasticity of the *aqueous vapour* generated from the powder in its combustion. The brilliant discoveries of modern chemists have taught us, that both the constituent parts of which water is composed, and even water itself, exist in the materials which are combined to make gunpowder; and there is much reason to believe that water is actually formed, as well as disengaged, in its combustion. M. Lavoisier, I know, imagined that the force of fired gunpowder, depends in a great measure upon the expansive force of uncombined *caloric*, supposed to be let loose in great abundance during the combustion or deflagration of the powder: but it is not only dangerous to admit the action of an agent whose existence is not yet clearly demonstrated, but it appears to me that this supposition is quite unnecessary; the elastic force of the heated aqueous vapour, whose existence can hardly be doubted, being quite sufficient to account for all the phænomena. It is well known that the elasticity of aqueous vapour is incomparably more augmented by any given augmentation of temperature, than that of any permanently elastic fluid whatever; and those who are acquainted with the amazing force of steam, when heated only to a few degrees

above the boiling point, can easily perceive that its elasticity must be almost infinite when greatly condensed and heated to the temperature of red-hot iron; and this heat it must certainly acquire in the explosion of gunpowder. But if the force of fired gunpowder arises *principally* from the elastic force of heated aqueous vapour, a cannon is nothing more than a *steam-engine* upon a peculiar construction; and upon determining the ratio of the elasticity of this vapour to its density, and to its temperature, a law will be found to obtain, very different from that assumed by Mr. ROBINS, in his Treatise on Gunnery. What this law really is, I do not pretend to have determined with that degree of precision which I wished; but the experiments of which I am about to give an account will, I think, demonstrate in the most satisfactory manner, not only that the force of fired gunpowder is in fact much greater than has been imagined, but also that its force consists principally in the temporary action of a fluid not permanently elastic, and consequently that all the theories hitherto proposed for the elucidation of this subject, must be essentially erroneous.

The first step towards acquiring knowledge is undoubtedly that which leads us to a discovery of the falsehood of received opinions. To a diligent inquirer every common operation, performed in the usual course of practice, is an experiment, from which he endeavours to discover some new fact, or to confirm the result of former inquiries.

Having been engaged many years in the investigation of the force of gunpowder, I occasionally found many opportunities of observing, under a variety of circumstances, the various effects produced by its explosion; and as a long habit of meditating upon this subject rendered every thing relating

to it highly interesting to me; I seized these opportunities with avidity, and examined all the various phænomena with steady and indefatigable attention.

During a cruise which I made as a volunteer in the Victory, with the British fleet, under the command of my late worthy friend Sir CHARLES HARDY, in the year 1779, I had many opportunities of attending to the firing of heavy cannon: for though we were not fortunate enough to come to a general action with the enemy, as is well known, yet, as the men were frequently exercised at the great guns, and in firing at marks, and as some of my friends in the fleet, then captains, (since made admirals) as the Honourable KEITH STEWART, who commanded the Berwick of 74 guns—Sir CHARLES DOUGLAS, who commanded the Duke of 98 guns—and Admiral MACBRIDE, who was then captain of the Bienfaisant of 64 guns, were kind enough, at my request, to make a number of experiments, and particularly by firing a greater number of bullets at once from their heavy guns than ever had been done before, and observing the distances at which they fell in the sea; I had opportunities of making several very interesting observations, which gave me much new light relative to the action of fired gunpowder. And afterwards, when I went out to America, to command a regiment of cavalry which I had raised in that country for the King's service, his Majesty having been graciously pleased to permit me to take out with me from England four pieces of light artillery, constructed under the direction of the late Lieutenant General DESAGULIERS, with a large proportion of ammunition, I made a great number of interesting experiments with these guns, and also with the

ship guns on board the ships of war in which I made my passage to and from America.

It would take up too much time, and draw out this paper to too great a length, to give an account in detail of all these experiments, and of the various observations I have had opportunities of making from time to time, relative to this subject. I shall, therefore, only observe at present, that the result of all my inquiries tended to confirm me more and more in the opinion, that the theory generally adopted relative to the explosion of gunpowder was extremely erroneous, and that its force is in fact much greater than is generally imagined. That the position of Mr. ROBINS, which supposes the inflammation and combustion of gunpowder to be so instantaneous "that "the whole of the charge of a piece of ordnance is actually "inflamed and converted into an elastic vapour before the "bullet is sensibly moved from its place," is very far from being true; and that the ratio of the elasticity of the generated fluid, to its density, or to the space it occupies as it expands, is very different from that assumed by Mr. ROBINS.

The rules laid down by Mr. ROBINS for computing the velocities of bullets from their weight, the known dimensions of the gun, and the quantities of powder made use of for the charge, may, and certainly do, very often give the velocities very near the truth; but this is no proof that the principles upon which these computations are made are just; for it may easily happen, that a complication of erroneous suppositions may be so balanced, that the result of a calculation founded on them may, nevertheless, be very near the truth; and this is never so likely to happen as when, from known effects, the

action of the powers which produce them are computed. For it is not in general very difficult to assume such principles as, when taken together, may in the most common known cases answer completely all the conditions required. But in such cases, if the truth be discovered with regard to any one of the assumed principles, and it be substituted in the place of the erroneous supposition, the fallacy of the whole hypothesis will immediately become evident.

As I have mentioned the experiments made with heavy artillery, as having been led by their results to form important conjectures relative to the nature of the expansion of the fluid generated in the combustion of gunpowder; it may perhaps be asked, and indeed with some appearance of reason, what the circumstances were which attended the experiments in question, which could justify so important a conclusion as that of the fallacy of the commonly received theory relative to that subject. To this I answer briefly, that in regard to the supposed instantaneous inflammation of the powder, upon which the whole fabric of this theory is built, or rather of all the computations which are grounded upon it, a careful attention to the phænomena which take place upon firing off cannon, led me to suspect, or rather confirmed me in my former suspicions, that however rapid the inflammation of gunpowder may be, its *total combustion* is by no means so sudden as this theory supposes. When a heavy cannon is fired in the common way, that is, when the vent is filled with loose powder, and the piece is fired off with a match, the time employed in the passage of the inflammation through the vent into the chamber of the piece is perfectly sensible, and this time is evidently shorter after the piece has been heated by

repeated firing. With the same charge, the recoil of a gun, (and consequently the velocity of its bullet), is greater after the gun has been heated by repeated firing than when it is cold. The velocity of the bullet is considerably greater when the cannon is fired off with a vent tube, or by firing a pistol charged with powder into the open vent, than when the vent is filled with loose powder. The velocity of two, three, or more fit bullets discharged at once from a piece of ordnance, compared to the velocity of one single bullet discharged by the same quantity of powder, from the same cannon, is greater than it ought to be according to the theory. Considerable quantities of powder are frequently driven out of cannon and other fire-arms *unconsumed*. The manner in which the smoke of gunpowder rises in the air, and is gradually dissolved and rendered invisible, shews it to partake of the nature of steam. But not to take up too much time with these general observations, I shall proceed to give an account of experiments the results of which will be considered as more conclusive.

Having found it impossible to measure the elastic force of fired gunpowder with any degree of precision by any of the methods before mentioned, I totally changed my plan of operations, and instead of endeavouring to determine its force by causing the generated elastic fluid to act upon a moveable body through a determined space, I set about contriving an apparatus in which this fluid should be made to act, by a determined surface, against a weight, which by being increased at pleasure should at last be such as would just be able to confine it, and which in that case would just counterbalance and consequently *measure* its elastic force.

The idea of this method of determining the force of fired

gunpowder occurred to me many years ago; but a very expensive and troublesome apparatus being necessary in order to put it in execution, it was not till the year 1792, when, being charged with the arrangement of the army of his most Serene Highness the ELECTOR PALATINE, reigning Duke of Bavaria, and having all the resources of the military arsenal, and a number of very ingenious workmen at my command, with the permission and approbation of his most Serene Electoral Highness, I set about making the experiments which I shall now describe: and as they are not only important in themselves, and in their results, but as they are, I believe, the first of the kind that have been made, I shall be very particular in my account of them, and of the apparatus used in making them.

One difficulty being got over, that of setting fire to the powder without any communication with the external air, by causing the heat employed for that purpose to pass through the solid substance of the barrel, it only remained to apply such a weight to an opening made in the barrel as the whole force of the generated elastic fluid should not be able to lift, or displace; but in doing this many precautions were necessary. For, first, as the force of gunpowder is so very great, it was necessary to employ an enormous weight to confine it; for, though by diminishing the size of the opening, the weight would be lessened in the same proportion, yet it was necessary to make this opening of a certain size, otherwise the experiments would not have been satisfactory; and it was necessary to make the support or base upon which the barrel was placed very massy and solid, to prevent the errors which would unavoidably have arisen from its want of solidity, or from its elasticity.

The annexed drawings (Tab. V.) will give a complete idea

of the whole apparatus made use of in these experiments. A. (fig. 1.) is a solid block of very hard stone, 4 feet 4 inches square, placed upon a bed of solid masonry, which descended 6 feet below the surface of the earth. Upon this block of stone, which served as a base to the whole machinery, was placed the barrel B of hammered iron, upon its support C, which is of cast brass, or rather of gun-metal; which support was again placed upon a circular plate of hammered iron D, 8 inches in diameter, and $\frac{1}{4}$ of an inch thick, which last rested upon the block of stone. The opening of the bore of the barrel (which was placed in a vertical position, and which was just $\frac{1}{4}$ of an inch in diameter) was closed by a solid hemisphere E of hardened steel, whose diameter was 1.16 inch; and upon this hemisphere the weight F, made use of for confining the elastic fluid generated from the powder in its combustion, reposed. This weight, (which in some of the most interesting experiments was a cannon of metal, a heavy twenty-four pounder, placed vertically upon its cascabel) being fixed to the timbers G G which formed a kind of carriage for it, was moveable up and down; the ends of these timbers being moveable in grooves cut in the vertical timbers K K, which being fixed below in holes made to receive them in the block of stone, and above by a cross piece L, were supported by braces and iron clamps made fast to the thick walls of building of the arsenal. This weight was occasionally raised and lowered in the course of the experiments (in placing and removing the barrel), by means of a very strong lever, which is omitted in the drawing to make it less complicated. The barrel, a section of which is represented in fig. 2. of its natural size, is 2.78 inches long, and 2.82 inches in diameter, at its lower extremity, where it

reposes upon its supporter, but something less above, being somewhat diminished, and rounded off at its upper extremity. Its bore, which, as I have already observed, is $\frac{1}{4}$ of an inch in diameter, is 2.13 inches long, and it ends in a very narrow opening below, not more than 0.07 of an inch in diameter, and 1.715 inch long, which forms the vent (if I may be permitted to apply that name to a passage which is not open at both ends), by which the fire is communicated to the charge. From the centre of the bottom of the barrel there is a projection of about 0.45 of an inch in diameter, and 1.3 inch long, which forms the vent tube V. Fig. 3. is a view of an iron ball W, which being heated red-hot, and being applied to the vent tube by means of an hole O made in it for that purpose, fire is communicated through the solid substance of the vent tube to the powder it contains, and from thence to the charge.

Fig. 4. which is drawn on a scale of two inches to the inch, or half the real size of the machinery, shews how the barrel B was placed upon its support C; how this last was placed upon its circular plate of iron D, and how the red-hot iron ball W was applied to the vent tube V. This ball is managed by means of a long handle *b* of iron, and being introduced through a circular opening *g* in the support, and applied to the vent tube V, is kept in its place by means of a wedge, or rather lever *l*, whose external end is represented in the drawing as being broken off, to save room. The circular opening in front of the support is seen in front, and consequently more distinctly, in the drawing, fig. 1. In this drawing the end of the vent tube may be likewise discovered through this opening; but as it was necessary, in order to introduce all the parts of this machinery, to make the drawing upon a very small scale, it was not possible

to express all the smaller parts with that distinctness which I wished. The other figures which are added, in which the parts are expressed separately, and upon a larger scale, will, it is hoped, supply this defect.

The stand, or support as I have called it, upon which the barrel was placed, is circular, and in order that it might be united more firmly to the plate of iron upon which it reposes, this plate is furnished with a cylindrical projection *p*, 1 inch long and $1\frac{1}{2}$ in diameter, which enters a hole made in the bottom of the stand to receive it.

Fig. 5. is a view of the barrel from above, in which the projecting screws, or rather cylinders, are seen, by which the hemisphere E, fig. 2. which closed the end of the barrel, was kept in its place. Two of these screws 1, 2, are seen in the figures 2 and 4. The smaller circle *a b*, fig. 5. shews the diameter of a circular plate of gold, which was let into the end of the barrel, being firmly fixed to the iron solder; and the larger circle *c d* represents a circular piece of oiled leather, which was placed between the end of the barrel and the hemisphere which rested upon it.

The end of the barrel was covered with gold, in order to prevent as much as possible its being corroded by the elastic vapour which, when the weight is not heavy enough to confine it, escapes between the end of the barrel and the flat surface of the hemisphere; but even this precaution was not found to be sufficient to defend the apparatus from injury. The sharp edge of the barrel at the mouth of the bore was worn away almost immediately, and even the flat surface of the hemisphere, notwithstanding it was of hardened steel and very highly polished, was sensibly corroded. This corrosion of the mouth of the

bore, by which the dimensions of the surface upon which the generated elastic fluid acted were rendered very uncertain, would alone have been sufficient to have rendered all my attempts to determine the force of fired gunpowder abortive, had I not found means to remedy the evil. The method I pursued for this purpose was as follows. Having provided some pieces of very good compact sole-leather, I caused them to be beaten upon an anvil with a heavy hammer, to render them still more compact; and then, by means of a machine made for that purpose, cylindric stoppers, of the same diameter precisely as the bore of the barrel, and 0.19 of an inch in length (that is to say, the thickness of the leather), were formed of it; and one of these stoppers, which had previously been greased with tallow, being put into the mouth of the piece after the powder had been introduced, and being forced into the bore till its upper end coincided with the end of the barrel, upon the explosion taking place, this stopper (being pressed on the one side by the generated elastic fluid, and on the other by the hemisphere, loaded with the whole weight employed to confine the powder), so completely closed the bore, that when the force of the powder was not sufficient to raise the weight to such a height that the stopper was actually blown out of the piece, not a particle of the elastic fluid could make its escape. And in those cases in which the weight was actually raised, and the generated elastic fluid made its escape, as it did not corrode the barrel in any other part but just *at the very extremity of the bore*, the experiment by which the weight was ascertained, which was just able to counterbalance the pressure of the generated elastic fluid, was in nowise vitiated, either by the increased diameter of the bore at its extremity, or by any

corrosion of the hemisphere itself; for as long as the bore retained its form and its dimensions, in that part to which the efforts of the elastic fluid were confined, that is, in that part of the bore immediately in contact with the lower part of the stopper, the experiment could not be affected by any imperfection of the bore either above or below.

In the figures 2. and 4. this stopper is represented in its place, and fig. 6. shews the plan, and fig. 7. the profile of one of these stoppers of its full size. Fig. 8. shews a small but very useful instrument, employed in introducing these stoppers into the bore, and more especially in occasionally extracting them: it resembles a common cork-screw, only it is much smaller. In the figure (where it is shewn in its full size), it is represented screwed into a stopper. Fig. 9. shews the plan, and fig. 10. a side view, of the hemisphere of hardened steel, by which the end of the barrel was closed. In the figures 2. and 4. the barrel is represented as being about half filled with powder.

Presuming that what has been already said, together with the assistance of the annexed drawings, will be sufficient to give a perfect idea of all the different parts of this apparatus, I shall now proceed to give an account of the experiments which from time to time have been made with it. And in order to render these details as intelligible as possible, and to shew the results of all these inquiries in a clear and satisfactory manner, I shall first give a brief account of the manner in which the experiments were made; of the various precautions used; and the particular appearances which were observed in the prosecution of them.

The powder made use of in these experiments was of the best quality, being that kind called *poudre de chasse* by the

French, and very fine grained: and it was all taken from the same parcel. Care was taken to dry it very thoroughly, and the air of the room in which it was weighed out for use was very dry. The weights employed for weighing the powder were German apothecary's grains, 104.8 of which make 100 grains Troy. I have reduced the weights employed to confine the elastic vapour generated in the combustion of the powder from Bavarian pounds, in which they were originally expressed, to pounds avoirdupois. The measures of length were all taken in English feet and inches. The experiments were all made in the open air, in the court-yard of the arsenal at Munich; and they were all made in fair weather, and between the hours of nine and twelve in the forenoon, and two and five in the afternoon; but the barrel was always charged, and the extremity of the bore closed by its leather stopper, in the room where the powder was weighed. In placing the barrel upon the block of stone, great care was taken to put it exactly under the centre of gravity of the weight employed to confine the generated elastic vapour. Upon applying the red-hot ball to the vent tube, and fixing it in its place by its lever which supported it, the explosion very soon followed.

When the force of the generated elastic vapour was sufficient to raise the weight, the explosion was attended by a very sharp and surprisingly loud report; but when the weight was not raised, as also when it was only a little moved, but not sufficiently to permit the leather stopper to be driven quite out of the bore, and the elastic fluid to make its escape, the report was scarcely audible at the distance of a few paces, and did not at all resemble the report which commonly attends the explosion of gunpowder. It was more like the noise

which attends the breaking of a small glass tube than any thing else to which I can compare it. In many of the experiments in which the elastic vapour was confined, this feeble report attending the explosion of the powder was immediately followed by another noise, totally different from it, which appeared to be occasioned by the falling back of the weight upon the end of the barrel, after it had been a little raised, but not sufficiently to permit the leather stopper to be driven quite out of the bore. In some of these experiments, a very small part only of the generated elastic fluid made its escape: in these cases the report was of a peculiar kind, and though perfectly audible at some considerable distance, yet not at all resembling the report of a musket. It was rather a very strong, sudden hissing, than a clear, distinct, and sharp report.

Though it could be determined with the utmost certainty by the report of the explosion, whether any part of the generated elastic fluid had made its escape, yet for still greater precaution, a light collar of very clean cotton wool was placed round the edge of the steel hemisphere, where it reposed upon the end of the barrel, which could not fail to indicate by the black colour it acquired, the escape of the elastic fluid, whenever it was strong enough to raise the weight by which it was confined sufficiently to force its way out of the barrel.

Though the end of the barrel at the mouth of the bore was covered with a circular plate of gold, in order the better to defend the mouth of the bore against the effects of the corrosive vapour, yet this plate being damaged in the course of the experiments (a piece of it being blown away), the remainder of it was removed; and it was never after thought necessary to replace it by another. When this plate of gold was taken

away, the length of the barrel was of course diminished as much as the thickness of this plate amounted to, which was about $\frac{1}{400}$ part of an inch; but in order that even this small diminution of the length of the barrel might have no effect on the results of the experiments, its bore was deepened $\frac{1}{400}$ of an inch when this plate was removed, so that the *capacity* of the bore remained the same as before.

After making use of a great variety of expedients, the best and most convenient method of closing the end of the bore, and defending the flat surface of the steel hemisphere from the corroding vapours, was found to be this; first, to cover the end of the bore with a circular plate of thin oiled leather, then to lay upon this a very thin circular plate of hammered brass, and upon this brass plate the flat surface of the hemisphere. When the elastic fluid made its escape, a part of the leather was constantly found to have been torn away, but never in more places than one; that is to say, always on one side only.

What was very remarkable in all those experiments in which the generated elastic vapour was completely confined, was the small degree of expansive force which this vapour appeared to possess after it had been suffered to remain a few minutes, or even only a few seconds, confined in the barrel; for, upon raising the weight by means of its lever, and suffering this vapour to escape, instead of escaping with a loud report, it rushed out with a hissing noise hardly so loud or so sharp as the report of a common air-gun; and its efforts against the leathern stopper, by which it assisted in raising the weight, were so very feeble as not to be sensible. Upon examining the barrel, however, this diminution of the force of the generated elastic fluid was easily explained; for what was undoubtedly in the

moment of the explosion in the form of an elastic fluid, was now found transformed into a *solid body* as hard as a stone! It may easily be imagined how much this unexpected appearance excited my curiosity; but, intent on the prosecution of the main design of these experiments, the ascertaining the force of fired gunpowder, I was determined not to permit myself to be enticed away from it by any extraordinary or unexpected appearances, or accidental discoveries, however alluring they might be; and faithful to this resolution, I postponed the examination of this curious phænomenon to a future period; and since that time I have not found leisure to engage in it. I think it right, however, to mention in this place such cursory observations as I was able, in the midst of my other pursuits, to make upon this subject; and it will afford me sincere pleasure, if what I have to offer should so far excite the curiosity of philosophers, as to induce some one who has leisure, and the means of pursuing such inquiries with effect, to precede me in the investigation of this interesting phænomenon; and as the subject is certainly not only extremely curious in itself, but bids fair to lead to other and very important discoveries, I cannot help flattering myself that some attention will be paid to it. I have said that the solid substance into which the elastic vapour generated in the combustion of gunpowder was transformed, was *as hard as a stone*. This I am sensible is but a vague expression; but the fact is, that it was very hard, and so firmly attached to the inside of the barrel, and particularly to the inside of the upper part of the vent tube, that it was always necessary, in order to remove it, to make use of a drill, and frequently to apply a considerable degree of force. This substance, which was of a black colour, or rather of a

dirty grey, which changed to black upon being exposed to the air, had a pungent, acrid, alkaline taste, and smelt like liver of sulphur. It attracted moisture from the air with great avidity. Being moistened with water, and spirit of nitre being poured upon it, a strong effervescence ensued, attended by a very offensive and penetrating smell. Nearly the whole quantity of matter of which the powder was composed, seemed to have been transformed into this substance; for the quantity of elastic fluid which escaped upon removing the weight, was very inconsiderable; but this substance was *no longer gunpowder*: it was not even inflammable. What change had it undergone? what could it have lost? It is very certain the barrel was considerably heated in these experiments. Was this occasioned by the *caloric*, disengaged from the powder in its combustion, making its escape through the iron? And is this a proof of the existence of *caloric*, considered as a fluid *sui generis*; and that it actually enters into the composition of inflammable bodies, or of pure air, and is necessary to their combustion? I dare not take upon me to decide upon such important questions. I once thought that the heat acquired by a piece of ordnance in being fired, arose from the vibration or friction of its parts, occasioned by the violent blow it received in the explosion of the powder; but I acknowledge fairly, that it does not seem to be possible to account in a satisfactory manner for the very considerable degree of heat which the barrel acquired in these experiments, merely on that supposition.

That this hard substance, found in the barrel after an experiment in which the generated elastic vapour had been completely confined, was actually in a fluid or elastic state in the moment of the explosion, is evident from hence, that in all

those cases in which the weight was raised, and the stopper blown out of the bore, nothing was found remaining in the barrel. It was very remarkable that this hard substance was not found distributed about in all parts of the barrel indifferently, but there was always found to be more of it near the middle of the length of the bore, than at either of its extremities; and the upper part of the vent tube in particular was always found quite filled with it. It should seem from hence, that it attached itself to those parts of the barrel which were soonest cooled; and hence the reason, most probably, why none of it was ever found in the lower part of the vent tube, where it was kept hot by the red-hot ball by which the powder was set on fire.

I found by a particular experiment, that the gunpowder made use of, when it was well shaken together, occupied rather less space in any given measure, than the same weight of water; consequently when gunpowder is fired in a confined space which it fills, the density of the generated elastic fluid must be at least equal to the density of water. The real specific gravity of the solid grains of gunpowder, determined by weighing them in air and water, is to the specific gravity of water, as 1.868 to 1.000. But if a measure, whose capacity is one cubic foot, hold 1000 ounces of water, the same measure will hold just 1077 ounces of fine grained gunpowder, such as I made use of in my experiments; that is to say, when it is well shaken together. When it was moderately shaken together, I found its weight to be exactly equal to that of an equal volume, or rather measure, of water. But it is evident that the weight of any given measure of gunpowder, must depend much upon the forms and sizes of its grains. I shall add only one

observation more, relative to the particular appearances which attended the experiments in which the elastic vapour generated in the combustion of gunpowder was confined, and that is, with regard to a curious effect produced upon the inferior flat surface of the leathern stopper, where it was in contact with the generated elastic vapour. Upon removing the stopper, its lower flat surface appeared entirely covered with an extremely white powder, resembling very light white ashes, but which almost instantaneously changed to the most perfect black colour upon being exposed to the air.

The sudden change of colour in this substance upon its being exposed to the air, has led me to suspect that the solid matter found in the barrel was not originally black, but that it became black merely in consequence of its being exposed to the air. The dirty grey colour it appeared to have immediately on being drilled out of the cavity of the bore, where it had fixed itself, seems to confirm this suspicion. An experiment made with a very strong glass barrel would not only decide this question, but would most probably render the experiment peculiarly beautiful and interesting on other accounts; and I have no doubt but a barrel of glass might be made sufficiently strong to withstand the force of the explosion. Whether it would be able to withstand the sudden effects of the heat, I own I am more doubtful; but as the subject is so very interesting, I think it would be worth while to try the experiment. Perhaps the apparatus might be so contrived as to set fire to the powder by the solar rays, by means of a common burning glass; but even if that method should fail, there are others equally unexceptionable, which might certainly be employed with success; and it is hardly possible to imagine any thing

more curious than an experiment of this kind would be, if it were successful.

But to proceed to the experiments by which I endeavoured to ascertain the force of fired gunpowder. All the parts of the apparatus being ready, it was in the autumn of the year 1798 that the first experiment was made.

The barrel being charged with 10 grains of powder (its contents when quite full amounting to about 28 grains), and the end of the barrel being covered by a circular piece of oiled leather, and the flat side of the hemisphere being laid down upon this leather, and a heavy cannon, a twenty-four pounder, weighing 8081 lbs. avoirdupois, being placed upon its cascabel in a vertical position upon this hemisphere, in order to confine by its weight the generated elastic fluid, the heated iron ball was applied to the end of the vent tube; and I had waited but a very few moments in anxious expectation of the event, when I had the satisfaction of observing that the experiment had succeeded. The report of the explosion was extremely feeble, and so little resembling the usual report of the explosion of gunpowder, that the by-standers could not be persuaded that it was any thing more than a cracking of the barrel, occasioned merely by its being heated by the red-hot ball: yet, as I had been taught by the result of former experiments not to expect any other report, and as I found upon putting my hand upon the barrel that it began to be sensibly warm, I was soon convinced that the powder must have taken fire; and after waiting four or five minutes, upon causing the weight which rested upon the hemisphere to be raised, the confined elastic vapour rushed out of the barrel. Upon removing the barrel and examining it, its bore was found to be choaked up by the solid

substance which I have already described, and from which it was with some difficulty that it was freed, and rendered fit for another experiment. The extreme feebleness of the report of the explosion, and the small degree of force with which the generated elastic fluid rushed out of the barrel upon removing the weight which had confined it, had inspired my assistants with no very favourable idea of the importance of these experiments. I had seen, indeed, from the beginning by their looks, that they thought the precautions I took to confine so inconsiderable a quantity of gunpowder as the barrel could contain, perfectly ridiculous; but the result of the following experiment taught them more respect for an agent, of whose real force they had conceived so very inadequate an idea.

In this second experiment, instead of 10 grains of powder, the former charge, the barrel was now quite filled with powder, and the steel hemisphere, with its oiled leather under it, was pressed down upon the end of the barrel by the same weight as was employed for that purpose in the first experiment, namely, a cannon weighing 8081 lbs. In order to give a more perfect idea of the result of this important experiment, it may not be amiss to describe more particularly one of the principal parts of the apparatus employed in it, I mean the barrel. This barrel (which though similar to it in all respects was not the same that has already been described,) was made of the best hammered iron, and was of uncommon strength. Its length was $2\frac{1}{4}$ inches; and though its diameter was also $2\frac{1}{4}$ inches, the diameter of its bore was no more than $\frac{1}{4}$ of an inch, or less than the diameter of a common goose quill. The length of its bore was 2.15 inches. Its diameter being $2\frac{1}{4}$ inches, and the diameter of its bore only $\frac{1}{4}$ of an inch, the thickness of the

metal was $1\frac{1}{4}$ inch; or, it was 5 times as thick as the diameter of its bore. The charge of powder was extremely small, amounting to but little more than $\frac{1}{10}$ of a cubic inch; not so much as would be required to load a small pocket pistol, and not *one-tenth part* of the quantity frequently made use of for the charge of a common musket. I should be afraid to relate the result of this experiment, had I not the most indisputable evidence to produce in support of the facts. This inconsiderable quantity of gunpowder, when it was set on fire by the application of the red-hot ball to the vent tube, exploded with such inconceivable force as to burst the barrel asunder in which it was confined, notwithstanding its enormous strength; and with such a loud report as to alarm the whole neighbourhood. It is impossible to describe the surprise of those who were spectators of this phænomenon. They literally turned pale with affright and astonishment, and it was some time before they could recover themselves. The barrel was not only completely burst asunder, but the two halves of it were thrown upon the ground in different directions: one of them fell close by my feet, as I was standing near the machinery to observe more accurately the result of the experiment. Though I thought it possible that the weight might be raised, and that the generated elastic vapour would make its escape, yet the bursting of the barrel was totally unexpected by me. It was a new lesson to teach me caution in these dangerous pursuits.

It affords me peculiar satisfaction in laying these accounts before the Royal Society, to be able to produce the most respectable testimony of their authenticity.

My friend Sir CHARLES BLAGDEN, one of the worthy Secretaries of the Society, visited Munich in the summer of the year

1793, in his return from Italy; and though I was then absent (travelling for the recovery of my health), yet, by my directions, he was not only shewn every part of the apparatus made use of in these experiments, but several experiments were actually repeated in his presence; and he was kind enough to take with him to England one half of the barrel which was burst in the experiment just mentioned, which at my request he has deposited in the Museum of the Society, and which I flatter myself will be looked upon as the most unequivocal proof of my discoveries relative to the amazing force of the elastic vapour generated in the combustion of gunpowder.

When the amazing strength of this barrel is considered, and when we consider the smallness of the capacity of its bore, it appears almost incredible, that so small a quantity of powder as that which was employed in the experiment could burst it asunder.

But without insisting on the testimony of several persons of respectable character, who were eye witnesses of the fact, and from whom Sir CHARLES BLAGDEN received a verbal account, in detail, of all the circumstances attending the experiment, I fancy I may very safely rest my reputation upon the silent testimony which this broken instrument will bear in my favour; much doubting whether it be in the power of art to burst asunder such a mass of solid iron, by any other means than those I employed.

Before I proceed to give an account of my subsequent experiments upon this subject, I shall stop here for a moment to make an estimate, from the known strength of iron, and the area of the fracture of the barrel, of the real force employed by the elastic vapour to burst it. In a course of experiments upon

the strength of various bodies which I began many years ago, and an account of which I intend at some future period to lay before the Royal Society,* I found, by taking the mean of the results of several experiments, that a cylinder of good tough hammered iron, the area of whose transverse section was only $\frac{3}{1000}$ of an inch, was able to sustain a weight of 119lbs. avoirdupois, without breaking. This gives 63,466 lbs. for the weight which a cylinder of the same iron whose transverse section is one inch, would be able to sustain without being broken. The area of the fracture of the barrel before mentioned was measured with the greatest care, and was found to measure very exactly $6\frac{1}{2}$ superficial inches. If now we suppose the iron of which this barrel was formed, to be as strong as that whose strength I determined (and I have no reason to suspect it to be of an inferior quality), in that case, the force actually employed in bursting the barrel must have been equal to the pressure of a weight of 412529 lbs. For the resistance or cohesion of one inch, is to 63466 lbs. as that of $6\frac{1}{2}$ inches to 412529 lbs.; and this force, so astonishingly great, was exerted by a body which weighed less than 26 grains Troy, and which acted in a space that hardly amounted to $\frac{1}{10}$ of a cubic inch.

To compare this force exerted by the elastic vapour gene-

* Since writing the above, I have met with a misfortune which has put it out of my power to fulfil my promise to the Royal Society. On my return to England from Germany in October, 1795, after an absence of eleven years, I was stopped in my post-chaise in St. Paul's churchyard, in London, at six o'clock in the evening, and robbed of a trunk which was behind my carriage, containing all my private papers and my original notes and observations on philosophical subjects. By this cruel accident I have been deprived of the fruits of the labours of my whole life; and have lost all that I held most valuable. This most severe blow has left an impression on my mind, which I feel that nothing will ever be able entirely to remove.

rated in the combustion of gunpowder, and by which the barrel was burst, to the pressure of the atmosphere, it is necessary to determine the area of a longitudinal section of the bore of the piece. Now the diameter of the bore being $\frac{1}{4}$ of an inch, and its length (after deducting 0.15 of an inch for the length of the leathern stoppers) 2 inches, the area of its longitudinal section turns out to have been $\frac{1}{2}$ an inch. And if now we assume the mean pressure of the atmosphere = 15 lbs. avoirdupois for each superficial inch, this will give $7\frac{1}{2}$ for that upon a surface = $\frac{1}{2}$ inch, equal to the area of a longitudinal section of the bore of the barrel.

But we have just found that the force actually exerted by the elastic vapour in bursting the barrel, amounted to 412529 lbs.; this force was therefore 55004 times greater than the mean pressure of the atmosphere! For it is as $7\frac{1}{2}$ lbs. to 1 atmosphere, so 412529 lbs. to 55004 atmospheres.

Thinking it might perhaps be more satisfactory to know the real strength of the identical iron of which the barrel used in the before mentioned experiment was constructed, rather than to rest the determination of the strength of the barrel upon the decision of the strength of iron taken from another parcel, and which very possibly might be of a different quality, since writing the above, I have taken the trouble to ascertain the strength of the iron of which the barrel was made, which was done in the following manner. Having the one half of the barrel still in my possession, I caused small pieces, 2 inches long, and about $\frac{1}{8}$ of an inch square, to be cut out of the solid block, in the direction of its length, with a fine saw; and these pieces being first made round in their middle by filing, and then by turning in a lathe with a very sharp instrument, were

reduced to such a size as was necessary, in order to their being pulled asunder in my machine for measuring the strength of bodies. In this machine the body to be pulled asunder is held fast by two strong vices, the one fastened to the floor, and the other suspended to the short arm of a Roman balance, or common steel-yard; and in order that the bodies so suspended may not be injured by the jaws of the vices, so as to be weakened and to vitiate the experiments, they are not made cylindrical, but they are made larger at their two ends where they are held by the vices, and from thence their diameters were gradually diminished towards the middle of their lengths, where their measures were taken, and where they never failed to break.

As I had found by the results of many experiments which I had before made upon the strength of the various metals, that iron, as well as all other metals, is rendered much stronger by hammering, I caused those pieces of the barrel which were prepared for these experiments to be separated from the solid block of metal, and reduced to their proper sizes, by sawing, filing, and turning, and without ever receiving a single blow of a hammer; so that there is every reason to believe that the strength of the iron, as determined by the experiments, may safely be depended on. The results of the experiments were as follows:

Experiments.	Diameter of the Cylinder at the Fracture.	Area of a transverse section of the Cylinder at the Fracture.	Weight required to break it. lbs. avoirdupois.	Weight required to break 1 inch of this iron. lbs. avoirdupois.
	Inch.	Inch.		
1.	$\frac{80}{1000}$	$\frac{1}{\text{50,29}}$	123.18	62737.
2.	$\frac{80}{1000}$	$\frac{1}{\text{213,68}}$	182.	64366.
3.	$\frac{80}{1000}$	$\frac{1}{\text{290, 3}}$	220.75	64526.
4.	$\frac{78}{1000}$	$\frac{1}{\text{220, 7}}$	277.01	61063.
		Number of Experiments = 4.)		252692.
			Mean	63173.

If now we take the strength of the iron of which the barrel was composed as here determined by actual experiments, and compute the force required to burst the barrel, it will be found equal to the pressure of a weight of 410624½ lbs. instead of 436800 as before determined. For it is the resistance or force of cohesion of 1 inch of this iron to 63173 lbs., as that of 6½ inches (the area of the fracture of the barrel) to 410624½ lbs. And this weight turned into atmospheres, in the manner above described, gives 54750 atmospheres for the measure of the force which must have been exerted by the elastic fluid in bursting the barrel. But this force, enormous as it may appear, must still fall short of the real initial force of the elastic fluid generated in the combustion of gunpowder, before it has begun to expand; for it is more than probable that the barrel was in fact burst before the generated elastic fluid had exerted all its force, or that this fluid would have been able to have burst a barrel still stronger than that used in the experiment.—But I wave these speculations in order to hasten to more interesting and more satisfactory investigations. Passing over in silence a consider-

able number of promiscuous experiments, which having nothing particularly remarkable in their results, could throw no new light upon the subject, I shall proceed immediately to give an account of a regular set of experiments, undertaken with a view to the discovery of certain determined facts, and prosecuted with unremitting perseverance.

These experiments were made by my directions under the immediate care of Mr. REICHENBACH, commandant of the corps of artificers in the Elector's military service, and of Count SPRETI, first lieutenant in the regiment of artillery.

Though I was prevented by ill health from being actually present at all these experiments, yet being at hand, and having every day, and almost every hour, regular reports of the progress that was made in them, and of every thing extraordinary that happened, the experiments may be said with great truth to have been made under my immediate direction; and as the two gentlemen by whom I was assisted, were not only every way qualified for such an undertaking, but had been present, and had assisted me in a number of similar experiments which I had myself made, they had acquired all that readiness and dexterity in the various manipulations which are so useful and necessary in experimental inquiries; and I think I can safely venture to say that the experiments may be depended upon. It would have afforded me great satisfaction to have been able to say that the experiments were all made by myself; and I had resolved to repeat them before I made them public, particularly as there appear to have been some very extraordinary and quite unaccountable differences in the results of those made in different seasons of the year; but having hitherto been prevented by ill health, and by other avocations, from engag-

ing again in these laborious researches, I have thought it right not to delay any longer the publication of facts, which appear to me to be both new and interesting, as their publication may perhaps excite others to engage in their farther investigation.

The principal objects I had in view in the following set of experiments were, first, to determine the expansive force of the elastic vapour generated in the combustion of gunpowder in its various states of condensation, and to ascertain the ratio of its elasticity to its density: and secondly, to measure, by one decisive experiment, the utmost force of this fluid in its most dense state; that is to say, when the powder completely fills the space in which it is fired, and in which the generated fluid is confined. As these experiments were very numerous, and as it will be more satisfactory to be able to see all their results at one cursory view, I have brought them into the form of a general table.

In this table, which does not stand in need of any particular explanation, may be seen the results of all these investigations.

The dimensions of the barrel made use of in the experiments mentioned in this table, were as follows.

Diameter of the bore at its muzzle = 0.25 of an inch.

Joint capacities of the bore, and of its vent tube, exclusive of the space occupied by the leathern stopper, = 0.08974 of a cubic inch.

Quantity of powder contained by the barrel and its vent tube when both were quite full, (exclusive of the space occupied by the leathern stopper,) 25.641 German apothecary's grains, = 24½ grains Troy.

The capacities of the barrel and of its vent tube were deter-

mined by filling them with mercury, and then weighing in air and in water the quantity of mercury required to fill them; and the quantity of powder required to fill the barrel and its vent tube was determined by computation, from the known joint capacities of the barrel and its vent tube, in parts of a cubic inch, and from the known specific gravity of the powder used in the experiments.

Thus the contents of the barrel and its vent tube having been found to amount to 0.08974 of a cubic inch, and it having been found that 1 cubic inch of the gunpowder in question, well shaken together, weighed just 272.68 grains Troy, this gives 24.47 grains Troy (= 25.641 grains, German apothecary's weight) for the contents of the barrel and its vent tube.

The numbers expressing the charges of powder in *thousandth parts* of the joint capacities of the barrel and of its vent tube, were determined from the known quantities of powder used in the different experiments, expressed in German apothecary's grains, and the relation of these quantities to the quantity required to fill the barrel and its vent tube completely.

Thus, as the barrel and its vent tube were capable of containing 25.641 apothecary's grains of powder, if we suppose this quantity to be divided into 1000 equal parts, this will give 39 of those parts for 1 grain; 78 parts for 2 grains; 390 for 10 grains, &c. For it is 25.641 to 1000, as 1 to 39 very nearly.

As this method of expressing the quantities of powder shows at the same time the relative density of the generated elastic fluid, it is the more satisfactory on that account: it will also

considerably facilitate the computations necessary in order to ascertain the ratio of the elasticity of this fluid to its density.

The elastic force of the fluid generated in the combustion of the charge of powder, is measured by the weight by which it was confined, or rather by that which it was just able to move, but which it could not raise sufficiently to blow the leathern stopper quite out of the mouth of the bore of the barrel.

This weight in all the experiments, except those which were made with very small charges of powder, was a piece of ordnance, of greater or less dimensions, or greater or less weight, according to the force of the charge; placed vertically upon its cascabel, upon the steel hemisphere which closed the end of the barrel; and the same piece of ordnance, by having its bore filled with a greater or smaller number of bullets, as the occasion required, was made to serve for several experiments.

The weight employed for confining the generated elastic fluid, is expressed in the following table in *pounds avoirdupois*; but in order that a clearer and more perfect idea may be formed of the real force of its elastic fluid, I have added a column in which its force, answering to each charge of powder, is expressed in *atmospheres*.

The numbers in this column were computed in the following manner. The diameter of the bore of the barrel at its muzzle being just $\frac{1}{4}$ of an inch, the area of its transverse section is 0.049088 of a superficial inch; and assuming the mean pressure of the atmosphere upon 1 superficial inch equal to 15lbs. avoirdupois, this will give 0.73631 of a pound avoirdupois for that pressure upon 0.049088 of a superficial inch, or upon a surface equal to the area by which the generated

elastic fluid acted on the weight employed to confine it; consequently the weight expressed in *pounds avoirdupois*, which measured the force of the generated elastic fluid in any given experiment, being divided by 0.78631, will show how many times the pressure exerted by the fluid was greater than the mean pressure of the atmosphere. Thus in the experiment, No. 6, where the weight which measured the elastic force of the generated fluid was = 504.8 lbs. avoirdupois, it is $\frac{504.8}{0.78631} = 685.6$ atmospheres. And so of the rest.

I have said that the diameter of the bore of the barrel, made use of in the following experiments, was just $\frac{1}{4}$ of an inch *at its muzzle*, and this is strictly true, as I found upon measuring it with the greatest care; but its diameter is not perfectly the same throughout its whole length, being rather narrower towards its lower end: yet the *capacity* of the barrel being known, and also *the diameter of the bore of its muzzle*, any small inequalities of the bore in any other part can in no wise affect the results of the experiments, as will be evident to those who will take the trouble to consider the matter for a moment with attention. I should not indeed have thought it necessary to mention this circumstance, had I not been afraid that some one who should calculate the joint capacities of the bore and of the vent tube from their lengths and diameters, finding their calculation not to agree with my determination of those capacities, as ascertained by filling them with mercury, might suspect me of having committed an error. The mean diameter of the bore of the barrel, as determined from its length and its capacity, turns out to be just 0.2281 of an inch; the diameter of the vent tube being taken equal to 0.07 of an inch, and its length 1.715 inch.

Table I. Experiments on the Force of fired Gunpowder.

No. of the Experiment.	Time when the Experiment was made. 1793.		State of the Atmosphere.		The charge of Powder.		Weight employed to confine the elastic Fluid.		General Remarks.
			Thermom.	Barometer.	In Apoth. gr.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In atmospheres.	
No	h.	m.	F.	Engl. In.	grs	Parts.	lbs.		
1	23d Feb. 9	0	31°	28.58	1	39	504.8		The generated elastic fluid was completely confined, the weight not being raised
2	9	30	—	—	2	78	—		
3	25th 9	0	37°	28.56	3	117	—		Ditto.
4	10	15	—	—	4	156	—		Ditto, weight not raised.
5	10	30	—	—	5	195	—		Ditto, ditto.
6	11	0	—	—	6	234	—	685.6	Weight just moved.
7	3	0 PM	57°	28.37	1	39	14.76		In these three experiments the weight was raised with a report as loud as that of a pistol.
8	3	15	—	—	—	—	26.5		
9	3	30	—	—	—	—	38.9		
10	3	45	—	—	—	—	51.3		But just raised, report much weaker.
11	4	0	—	—	—	—	57.4	77.86	Weight hardly moved.
12	26th 9	0	34°	28.1	2	78	103.5		Not raised.
13	9	15	—	—	—	—	124		Raised with a loud report.
14	9	30	—	—	—	—	130.5		Ditto, the report weaker.
15	9	45	—	—	—	—	133		Ditto, the report still weaker.
16	10	0	—	—	—	—	134.2	182.3	Weight but just moved.
17	3	0	48°	28.31	3	117	186.3		Raised with a loud report.
18	3	15	—	—	—	—	198.7		Ditto, ditto.
19	3	30	—	—	—	—	204.8		Ditto, report weaker.
20	3	45	—	—	—	—	208.5		Raised, report weaker.
21	4	0	—	—	—	—	212.24	288.2	The weight hardly moved, no report.
22	27th 3	0	50°	28.36	4	156	269.2		Raised with a loud report.
23	3	15	—	—	—	—	274.13		Ditto, ditto.
24	3	30	—	—	—	—	277.9		Ditto, report less loud.
25	3	45	—	—	—	—	281.57	382.4	Weight hardly moved, and no report.
26	28th 9	0	34°	28.32	5	195	319.68		Raised, loud report.
27	9	15	—	—	—	—	351.37		Ditto, ditto.
28	9	30	—	—	—	—	400.9		Ditto, ditto.
29	10	0	—	—	—	—	475.2		Not raised.
30	3	0	48°	28.35	—	—	443.5		Not raised.
31	3	15	—	—	—	—	425.65		Not raised.
32	3	30	—	—	—	—	419.46		Not raised.

Table I. Experiments on the Force of fired Gunpowder.

No. of the Experiment.	Time when the Experiment was made. 1793.		State of the Atmosphere.		The charge of Powder.		Weight employed to confine the elastic Fluid.		General Remarks.
			Thermom.	Barometer.	In Apoth. gr.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In atmospheres.	
N°	h.	m.	F.	Eng. In.	grs	Parts.	lbs.		
33	28th Feb. 3	45	48°	28.35	5	195	413.27	561.2	Weight but just moved.
34	1st Mar. 9	0	34°	28.35	7	273	535.79		Raised with a loud report.
35	9	15	—	—		—	548.14		Ditto, ditto.
36	9	30	—	—		—	560.52		Ditto, ditto.
37	3	0	59°	28.34		—	572.9		Ditto, ditto.
38	3	15	—	—		—	585.28		Ditto, report weaker.
39	3	30	—	—		—	597.66	811.7	Weight but just moved, no report.
40	3	45	—	—	8	312	690.52		Raised, report very loud.
41	4	0	—	—		—	752.42		Ditto, ditto.
42	4	15	—	—		—	783.37		Ditto, ditto.
43	2d 9	0	50°	28.32		—	876.22		Not raised.
44	9	15	—	—		—	845.19		But just raised, report weak.
45	9	30	—	—		—	857.64	1164.8	Weight but just moved, and no report.
46	9	45	—	—	9	351	961.65		Raised with a loud report.
47	10	0	—	—		—	1209.4		Not raised.
48	10	30	—	—		—	1142.3	1551.3	Weight just moved, no report.
49	3	0	52°	28.33	10	390	1456.8		Not raised.
50	3	30	—	—		—	1329.9		Raised, loud report.
51	5th 9	0	32°	28.2		—	1387.5	1884.3	Weight but just moved, and no report.
52	9	15	—	—	11	429	1708.2		Not raised.
53	9	45	—	—		—	1646.2		Not raised.
54	10	15	—	—		—	1615.2		Raised, with a weak report.
55	10	45	—	—		—	1634	2219	Weight but just moved, and no report.
56	6th 9	0	36°	28.34	12	468	1943.3		Not raised.
57	9	30	—	—		—	1932.2		Not raised.
58	10	30	—	—		—	1907.4		Weight not raised.
59	11	0	—	—		—	1878.4		Raised with a loud report.
60	11	30	—	—		—	1895.1	2573.7	Weight but just moved, and no report.
61	3	0	42°	28.3	13	507	2142.7		Raised with a loud report.
62	3	15	—	—		—	2204.6		Ditto, ditto.

Table I. Experiments on the Force of fired Gunpowder.

No. of the Experiment.	Time when the Experiment was made, 1793.		State of the atmosphere.		The charge of Powder.		Weight employed to confine the elastic Fluid.		General Remarks.
			Thermom.	Barometer.	Apoth. grs.	In 1000 parts of the grs. of powder.	In lbs. avoirdupois.	In atmospheres.	
N°	h.	m.	F.	Ens. In.	grs	Parts.	lbs.		
63	6th Mar. 3	30	42°	28.3	13	507	2266.5		Raised with a loud report.
64	3	45	—	—	—	—	2390.3		Raised, report weaker.
65	4	0	—	—	—	—	2422	3288.3	Weight just moved, no report.
66	9th 9	0	43°	28.31	14	546	3213		Not raised.
67	9	30	—	—	—	—	3093		Not raised.
68	10	0	—	—	—	—	2968		Not raised.
69	10	30	—	—	—	—	2846		Raised, with a loud report.
70	10	45	—	—	—	—	2908		Raised, report weaker.
71	11	0	—	—	—	—	2939		Ditto, report still weaker.
72	11	15	—	—	—	—	2951	4008	Weight but just moved, no report.
73	11	30	—	—	15	585	3750		Not raised.
74	11	45	—	—	—	—	3508		Not raised.
75	12	15	—	—	—	—	3477	4722.3	Weight but just moved, and no report.
76	11th 9	0	43°	28.3	16	624	4037		The weight was raised with a loud report.
77	9	15	—	—	—	—	4284		Raised, loud report.
78	9	30	—	—	—	—	4532		Ditto, ditto.
79	4th Apr. 3	0	70°	28.2	—	—	5027		Ditto, ditto.
80	3	15	—	—	—	—	5138		Raised, report weaker.
81	3	30	—	—	—	—	5202		Not raised.
82	3	45	—	—	—	—	5220	7090	Weight just moved, but no report.
83	5th 3	0	68°	28.3	17	663	8081		Not raised.
84	3	30	—	—	18	702	8081	10977	The weight was raised with a very sharp report, louder than that of a well loaded musket.
85	4	0	—	—	—	—	8700		The vent tube of the barrel was burst, the explosion being attended with a very loud report.

The barrel being rendered unfit for further service, by the bursting of its vent tube, an end was put to this set of experiments.

In order that a clear and satisfactory idea may be formed of the results of these experiments I have drawn the figure (Tab. VI.), in which the given densities of the generated elastic fluid, or (which amounts to the same thing) the quantities of powder used for the charge, being taken on the line A B, from A towards B, the corresponding elasticities, as found by the experiments, are represented by lines perpendicular to the line A B, at the points where the measures of the densities end.

As the irregularities of the dotted line A C are owing, no doubt, merely to the errors committed in making the experiments, these irregularities being removed, by drawing the line A D in such a manner as to balance the errors of the experiments, this line A D, which must necessarily be regular, will, by bare inspection, give us a considerable degree of insight into the nature of the equation which must be formed to express the relation of the densities to the elasticities; one principal object of these experimental inquiries.

Putting the density $= x$, and the elasticity $= y$, the line A D will be the locus of the equation expressing the relation of x to y; and had Mr. ROBINS's supposition, that the elasticity is as the density, been true, x would have been found to be to y in a constant (simple) ratio, A D would have been a straight line, and A E would have been the position of this line, had Mr. ROBINS's determination of the force of fired gunpowder been accurate.

But A D is a curve, and this shows that the ratio of x to y

is variable; and moreover it is a curve *convex towards the line* A B, on which x is taken; and this circumstance proves that the ratio of y to x is continually increasing.

Though these experiments all tend to show that the ratio of y to x increases as x is increased, yet when we consider the subject with attention, we shall, I think, find reason to conclude that the exponent of that ratio can never be less than *unity*; and farther, that it must of necessity have *that value precisely*, when, the density being taken infinitely small, or $= 0$, x and y vanish together.

Supposing this to be the case, namely, that the exponent of the ultimate ratio of y to x is $= 1$, let the densities or successive values of x be expressed by a series of natural numbers,

0, 1, 2, 3, 4, &c. to 1000,

the last term $= 1000$ answering to the greatest density; or when the powder completely fills the space in which it is confined; then, by putting $z =$ the variable part of the exponent of the ratio of y to x,

To each of the successive values of $x =$ } 0, 1, 2, 3, 4, &c.

The corresponding value of y will be accurately expressed by the equations } 0^{1+z}, 1^{1+z}, 2^{1+z}, 3^{1+z}, 4^{1+z}, &c.

For, as the variable part (z), of this exponent may be taken of *any dimensions*, it may be so taken at each given term of the series, (or for each particular value of x), that the equation $x^{1+z} = y$, may always correspond with the result of the experiments; and when this is done, the value of z, and the law of its increase as x increases, will be known; and this will show the relation of x to y, or of the elasticities of the ge-

nerated fluid to their corresponding densities, in a clear and satisfactory manner.

Without increasing the length of this paper still more (it being perhaps already too voluminous), by giving an account in detail of all the various computations I made, in order, from the results of the experiments in the foregoing table, to ascertain the real value of z, and the rate at which it increases as x is increased, I shall content myself with merely giving the general results of these investigations, and referring for farther information to the following table II, where the agreement of the law founded on them, with the results of the foregoing experiments, may be seen.

Having from the results of the experiments in table I. computed the different values of z, corresponding to all the different densities, or different charges of powder, from 1 grain, or 39 *thousandth parts*, to 18 grains, or 702 *thousandth parts* of the capacity of the barrel, I found that while the density of the elastic fluid $= x$, expressed in *thousandth parts*, is increased from 0 to 1000 (or till the powder completely fills the space in which it is confined), the variable part z of the exponent of x, $(1 + z)$ is increased from 0 to $\frac{4}{10}$. And though some of the experiments, and particularly those which were made with large charges of powder, seemed to indicate that while x is increased with an equable or uniform motion, z increases with a motion continually accelerated; *yet*, as the results of by far the greatest number of the other experiments showed the velocity of the increase of z to be *equable*, this circumstance, added to some other reasons drawn from the nature of the subject, have induced me to assume the ratio of the increase of z to the increase of x as constant.

But if, while x increases with an equable velocity from 0 to 1000, z is increased with an *equable velocity* from 0 to $\frac{4}{10}$, then it is every where z to x as $\frac{4}{10}$ to 1000; or $1000z = \frac{4}{10}x$, and consequently $z = \frac{4x}{10000}$; and when x is $= 1$, it is $z = \frac{4}{10000} = 0.0004$; and when x is greater or less than 1, it is $z = 0.0004x$; and z being expunged, the general equation expressing the relation of x to y becomes $x^{1+0.0004x} = y$; and this is the equation which was made use of in computing the values of y, as expressed in the following table.

In order that the elasticities might be expressed in atmospheres, the values of y, as determined by this equation, were multiplied by 1.841.

If it be required to express the elasticity in *pounds avoirdupois*, then the value of y, as determined by the foregoing equation, being multiplied by 27.615, will show how many pounds avoirdupois, pressing upon a superficial inch, will be equal to the pressure exerted by the elastic fluid in the case in question.

Table II. General Results of the Experiments in Table I. on the Force of fired Gunpowder.

The Charge of Powder.		Value of the Exponent $1+0.0004x$.	Computed Elasticity of the generated Fluid, or Value of y, according to the Theorem $x^{1+0.0004x}=y$.		Actual Elasticity, as shown by the Experiments.	Difference of the computed and the actual Elasticities.
In Grains.	In equal Parts.		In equal Parts.	In Atmospheres.	In Atmospheres.	In Atmospheres.
1	39	1.0156	41.294	76.822	77.86	+ 1.838
2	78	1.0312	89.357	164.506	182.30	+ 17.794
3	117	1.0468	146.210	269.173	228.2	− 40.973
4	156	1.0624	213.784	393.577	382.4	− 11.177
5	195	1.0780	294.209	541.640	561.2	+ 19.560
6	234	1.0936	389.919	717.841	685.6	− 32.241
7	273	1.1092	503.723	927.353	811.7	− 115.653
8	312	1.1248	638.889	1176.19	1164.8	− 12.390
9	351	1.1404	799.223	1471.37	1551.3	+ 79.930
10	390	1.1560	989.169	1821.06	1884.3	+ 63.240
11	429	1.1716	1213.91	2234.81	2219.	− 15 810
12	468	1.1872	1479.50	2723.77	2573.7	− 150.07
13	507	1.2028	1793.	3300.91	3283.3	− 17.61
14	546	1.2184	2162.69	3980.52	4008.	+ 27.48
15	585	1.2340	2598.18	4783.26	4722.5	− 60.76
16	624	1.2496	3110.73	5726.83	7090.	+1363.17
17	663	1.2652	3713.46	6836.46		
18	702	1.2808	4421.69	8140.34	10977.	+2836.66
19	741	1.2964	5253.3	9671.33		
20	780	1.3120	6229.14	11467.8		
25.641	1000	1.4000	15848.9	29177.9		

The agreement of the elasticities computed from the theorem $x^{1+0.0004x}=y$, with the actual elasticities as they were measured in the experiments, may be seen in the foregoing table; but this agreement may be seen in a much more striking manner by a bare inspection of the figure (Tab. VI.); for the line A D in this figure having been drawn from the computed elasticities, its general coincidence with the line A C shows how nearly the computed and the actual elasticities approach each other. And

when the irregularities of the line AC (which, as had already been observed, must be attributed to the unavoidable errors of the experiments), are corrected, these two curves will be found to coincide with much precision throughout a considerable part of the range of the experiments; but towards the end of the set of experiments, when the charges of powder were considerably increased, the elasticities seem to have increased faster than, according to the assumed law, they ought to have done. From this circumstance, and from the immense force the charge must have exerted in the experiment, when the barrel was burst, I was led to suspect that the elastic force of the fluid generated in the combustion of gunpowder, when its density is great, is still much greater than these experiments seem to indicate; and a farther investigation of the subject served to confirm me in this opinion.

It has been shown that the force exerted by the charge in the experiment in which the barrel was burst could not have been less than the pressure of 54,752 atmospheres; but the greatest force of the generated elastic fluid, when, the powder filling the space in which it is confined, its density is $= 1000$, on computing its elasticity by the theorem $x^{1+0.0004x} = y$, turns out to be only equal to 29,178 atmospheres.

In this computation the mean of the results of all the experiments in the foregoing set is taken as a standard to ascertain the value, expressed in atmospheres, of y, and it is $y \times 1.841 = 29,178$.

But if, instead of taking the mean of the whole set of experiments as a standard, we select that experiment in which the force exerted by the powder appears to have been the greatest,

yet in this case even the initial force of fired gunpowder, computed by the above rule, would be much too small.

In the experiment No. 84, when the charge consisted of 18 grains of powder, and the density or value of x was 702, a weight equal to the pressure of 10,977 atmospheres was raised. Here the value of y ($= x^{1+0.000417}$) is found to be ($702^{1.2828}$), $= 4421.7$; and to express this value of y in atmospheres, and at the same time to accommodate it to the actual result of the experiment, it must be multiplied by 2.4826; for it is 4421.7 (the value of y expressed in equal parts) to 10,977 (its value in atmospheres, as shown by the experiment), as 1 to 2.4826, and consequently $4421.7 \times 2.4826 = 10,977$.

If now the value of y be computed on the same principles, when x is put $= 1000$, it will turn out to be $y = 1000^{1+0.4} = 15,849$; and this number expressed in atmospheres, by multiplying it by 2.4826, gives the value of $y = 39,346$ atmospheres.

This however falls still far short of 54,752 atmospheres, the force the powder was actually found to exert when the charge filled the space in which it was confined. But in the 84th experiment, when 18 grains of powder were used, as the weight (8081 lbs. avoirdupois) was raised with *a very loud report*, it is more than probable that the force of the generated elastic fluid was in fact considerably greater than that at which it was estimated, namely, greater than the pressure of 10,977 atmospheres.

But, without wasting time in fruitless endeavours to reconcile anomalous experiments, which, probably, never can be made to agree, I shall hasten to give an account of another

set of experiments; the results of which, it must be confessed, were still more various, extraordinary, and inexplicable.

The machinery having been repaired and put in order, the experiments were recommenced in July, 1793, the weather at that time being very hot.

The principal part of the apparatus, *the barrel*, had undergone a trifling alteration: upon refitting and cleaning it, the diameter of its bore at the muzzle was found to be a little increased, so that a weight equal to 8081lbs. avoirdupois, instead of being equal to 10977 atmospheres (as was the case in the former experiments), was now just equal to the pressure of 9431 atmospheres.

Though I was not at Munich when this last set of experiments was made, they however were undertaken at my request, and under my direction, and I have no reason to doubt of their having been executed with all possible care. They were all made by the same persons who were employed in making the first set; and as these experimenters may be supposed to have grown expert in practice, and as they could not possibly have had any interest in deceiving me, I cannot suspect the accuracy of their reports.

Table III. Experiments on the Force of fired Gunpowder.

No. of the Experiment.	Time when the Experiment was made. 1793.		State of the Atmosphere.		The Charge of Powder.		Weight employed to confine the elastic Fluid.		General Remarks.
			Thermom.	Barometer.	In Apoth. grs.	In 1000 parts of the capacity of the Bore.	In lbs. avoir. dupois.	In atmospheres	
N°		h. m.	F.	Eng. In.	grs.	Parts.	lbs.		
86	1st July	4 0	88°	28.37	17	663	8081	9431	The weight was raised with an astonishing loud report.
87		4 30	—	—	—	—	—		
88		4 45	—	—	16	624	—		In these three experiments the weight was raised with a very loud report.
89		5 0	—	—	15	585	—		
90		5 30	—	—	12	468	—		Weight not raised.
91		6 0	—	—	13	507	—	9431	Weight but just raised, report very weak.
92	2d	9 0	71°	28.38	—	—	—		Raised, loud report.
93		9 30	—	—	12	468	—		Raised, feeble report.
94		10 0	—	—	—	—	—	9431	Raised, report very feeble.
95		10 30	80°	—	11½	—	—		*Just moved*, no report.
96	3d	10 0	76°	28.55	12	468	—		Not raised.
97		10 30	—	—	13	507	—		Not raised.
98		11 0	75°	—	14	546	—	9431	Just raised, feeble report.
99	4th	9 0	70°	28.56	14	546	—		Not raised.
100		9 30	—	—	—	—	—		Not raised.
101		10 0	72°	—	15	585	—		The weight was raised, the report not very loud.
102		10 30	—	—	15½	—	—		Nearly as above.
103	8th	9 0	74°	28.42	—	—	—		Raised, and with an uncommonly loud report.
104		9 30	—	—	13	507	—		Raised, report very loud.
105		10 45	85°	—	12	468	—	9431	But just raised, the report very feeble.
106	17th	9 0	75°	28.4	—	—	—		Nearly as above.
107		9 45	—	—	—	—	—		Not raised.
108		10 30	—	—	11½	—	—		*Just moved*, no report.
109		11 0	—	—	—	—	—		The same as above.

It appears from the foregoing table, that in the afternoon of the 1st of July, the weight (which was a heavy brass cannon, a 24 pounder, weighing 8081 lbs. avoirdupois), was not raised by 12 grains of powder, but that 13 grains raised it with an audible though weak report. That the next morning, July 2d, at 10 o'clock, it was raised twice by charges of 12 grains. That in the morning of the 3d of July, it was not raised by 12 grains, nor by 13 grains; but that 14 grains just raised it. That in the afternoon of the same day, two experiments were made with 14 grains of powder, in neither of which the weight was raised; but that in another experiment, in which 13 grains of powder were used, it was raised with a moderate report. That in the morning of the 8th July, in two experiments, one with 15 grains, and the other with 13 grains of powder, the weight was raised with a *loud report*; and in an experiment with 12 grains, it was raised with a *feeble report*. And lastly, that in three successive experiments, made in the morning of the 17th of July, the weight was raised by charges of 12 grains.

Hence it appears, that under circumstances the most favourable to the developement of the force of gunpowder, a charge (= 12 grains) filling $\frac{111}{1000}$ of the cavity in which it is confined, on being fired, exerts a force against the sides of the containing vessel equal to the pressure of 9431 atmospheres; which pressure amounts to 141465 lbs. avoirdupois on each superficial inch.

Mr. Robins makes the initial, or greatest force of the fluid generated in the combustion of gunpowder, (namely when the charge completely fills the space in which it is confined), to

be only equal to the pressure of 1000 atmospheres. It appears, however, from the result of these experiments, that even admitting the elasticities to be as the densities, as Mr. ROBINS supposes them to be, the initial force of this generated elastic fluid must be at least twenty times greater than Mr. ROBINS determined it; for $\frac{444}{1000}$, the density of the elastic fluid in the experiments in question, is to 1, its density when the powder quite fills the space in which it is confined, as 9481 atmospheres, the measure of its elastic force in the experiments in question, to 20108 atmospheres; which, according to Mr. ROBINS's theory respecting the ratio of the elasticities to the densities, would be the measure of its initial force.

But all my experiments tend uniformly to prove, that the elasticities increase *faster* than in the simple ratio of the corresponding densities; consequently the initial force of the generated elastic fluid *must necessarily* be greater than the pressure of 20108 atmospheres.

In one of my experiments which I have often had occasion to mention, the force actually exerted by the fluid must have been at least equal to the pressure of 54752 atmospheres. The other experiments ought, no doubt, to show, at least, that it is *possible* that such an enormous force may have been exerted by the charge made use of; and this, I think, they actually indicate.

In the first set of experiments, which were made when the weather was cold, though the results of them uniformly showed the force of the powder to be much less than it appeared to be in all the subsequent experiments, made with greater charges, and in warm weather, yet they all show that the ratio

of the elasticity of the generated fluid to its density is very different from that which Mr. Robins's theory supposes; and that this ratio increases as the density of the fluid is increased.

Supposing (what on many accounts appears to be extremely probable) that this ratio increases uniformly, or with an equable celerity, while the density is uniformly augmented; and supposing farther, that the velocity and limit of its increase have been rightly determined from the result of the set of experiments, table I. which were made with that view; then, from the result of the experiments of which we have just been giving an account, (in which 12 grains of powder exerted a force equal to 9431 atmospheres), taking these experiments as a standard, we can with the help of the theorem ($x^{1+0.0004x} = y$) deduced from the former set of experiments, compute the initial force of fired gunpowder, thus:

The density of the elastic fluid, when 12 grains of powder are used for the charge, being $= 468$, it is $468^{1.1875} = y = 1479.5$; and in order that this value of y may correspond with the result of the experiment, and be expressed in atmospheres, it must be multiplied by a certain coefficient, which will be found by dividing the value of y expressed in atmospheres, as shown by the experiment, by the number here found indicating its value, as determined by computation.

It is therefore $\frac{9411}{1479.5} = 6.3744$ for the value of this coefficient, and this multiplied into the number 1479.5 gives 9431 for the value of y in atmospheres.

Again, the density being supposed $= 1000$ (or, that the charge of powder completely fills the cavity in which it is confined), in that case it will be $1000^{1+0.4} = y = 15849$; and this number being turned into atmospheres by being multiplied by

the coefficient above found (= 6.3744), gives 101021 atmospheres for the measure of the initial force of the elastic fluid generated in the combustion of gunpowder.

Enormous as this force appears, I do not think it over-rated; for nothing much short of such an inconceivable force can, in my opinion, ever explain in a satisfactory manner the bursting of the barrel so often mentioned; and to this we may add, that, as in 7 different experiments, all made with charges of 12 grains of powder, there were no less than 5 in which the weight was *raised with a report*, and as the same weight was *moved* in 3 different experiments in which the charge consisted of less than 12 grains, there does not appear to be any reason whatever for doubt with regard to the principal fact on which the above computation is founded.

There is an objection, however, that may be made to these decisions respecting the force of gunpowder, which, on the first view, appears of considerable importance; but on a more careful examination it will be found to have no weight.

If the force of fired gunpowder is so very great, how does it happen that fire-arms and artillery of all kinds, which certainly are not calculated to withstand so enormous a force, are not always burst when they are used? I might answer this question by another, by asking how it happened that the barrel used in my experiments, and which was more than ten times stronger in proportion to the size of its bore than ever a piece of ordnance was formed, could be burst by the force of gunpowder, if its force is not in fact much greater than it has ever been supposed to be? But it is not necessary to have recourse to such a shift to get out of this difficulty: there is nothing more to do than to show, which may easily be done, that the combustion of

gunpowder is less rapid than it has hitherto been supposed to be, and the objection in question falls to the ground.

Mr. ROBINS's theory supposes that all the powder of which a charge consists is not only set on fire, but that it is actually *consumed* and "*converted into an elastic fluid before the bullet* "*is sensibly moved from its place.*" I have already in the former part of this paper offered several reasons which appeared to me to prove that, though the *inflammation* of gunpowder is very rapid, yet the progress of the combustion is by no means so *instantaneous* as has been imagined. I shall now give an account of some experiments which put that matter out of all doubt.

It is a fact well known that on the discharge of fire-arms of all kinds, cannon and mortars as well as muskets, there is always a considerable quantity of unconsumed grains of gunpowder blown out of them; and, what is very remarkable, and as it leads directly to a discovery of the cause of this effect is highly deserving of consideration, these unconsumed grains are not merely blown out of the *muzzles* of fire-arms; they come out also by their vents or touch-holes, *where the fire enters to inflame the charge;* as many persons who have had the misfortune to stand with their faces near the touch-hole of a musket, when it has been discharged, have found to their cost.

Now it appears to me to be extremely improbable, if not absolutely impossible, that a grain of gunpowder actually in the chamber of the piece, and completely surrounded by flame, should, by the action of that very flame, be blown out of it, without being at the same time set on fire. But if these grains of powder are *actually on fire* when they come out of the piece, and are afterwards found at a distance from it *unconsumed*,

this is, in my opinion, a most decisive proof, not only that the combustion of gunpowder is by no means so rapid as it has generally been thought to be, but also (what will doubtless appear quite incredible), that if a grain of gunpowder, actually on fire, and burning with the utmost violence over the whole extent of its surface, be projected with *a very great velocity* into a cold atmosphere, the fire will be extinguished, and the remains of the grain will fall to the ground unchanged, and as inflammable as before.

This extraordinary fact was ascertained beyond all possibility of doubt by the following experiments. Having procured from a powder-mill in the neighbourhood of the city of Munich a quantity of gunpowder, all of the same mass, but formed into grains of very different sizes, some as small as the grains of the finest Battel powder, and the largest of them nearly as big as large pease, I placed a number of vertical screens of very thin paper, one behind another, at the distance of 12 inches from each other; and loading a common musket repeatedly with this powder, sometimes without, and sometimes with a wad, I fired it against the foremost screen, and observed the quantity and effects of the unconsumed grains of powder which impinged against it.

The screens were so contrived, by means of double frames united by hinges, that the paper could be changed with very little trouble, and it was actually changed after every experiment.

The distance from the muzzle of the gun to the first screen was not always the same; in some of the experiments it was only 8 feet, in others it was 10, and in some 12 feet.

The charge of powder was varied in a great number of dif-

ferent ways, but the most interesting experiments were made with one single large grain of powder, propelled by smaller and larger charges of very fine-grained powder.

These large grains never failed to reach the screen; and though they sometimes appeared to have been broken into several pieces, by the force of the explosion, yet they frequently reached the first screen entire; and sometimes passed through all the screens (five in number), without being broken.

When they were propelled by large charges, and consequently with great velocity, they were seldom on fire when they arrived at the first screen, which was evident not only from their not setting fire to the paper (which they sometimes did), but also from their being found sticking in a soft board, against which they struck, after having passed through all the five screens; or leaving visible marks of their having impinged against it, and being broken to pieces and dispersed by the blow. These pieces were often found lying on the ground; and from their forms and dimensions, as well as from other appearances, it was often quite evident that the little globe of powder had been on fire, and that its diameter had been diminished by the combustion, before the fire was put out on the globe being projected into the cold atmosphere. The holes made in the screen by the little globe in its passage through them, seemed also to indicate that its diameter had been diminished.

That these globes or large grains of powder were always set on fire by the combustion of the charge can hardly be doubted. This certainly happened in many of the experiments, for they arrived at the screens on fire, and set fire to the paper; and in the experiments in which they were projected with small

velocities, they were often seen to pass through the air on fire; and when this was the case no vestige was to be found.

They sometimes passed, on fire, through several of the foremost screens without setting them on fire, and set fire to one or more of the hindmost, and then went on and impinged against the board, which was placed at the distance of 12 inches behind the last screen.

It is hardly necessary for me to observe, that all these experiments prove that the combustion of gunpowder is very far from being so instantaneous as has generally been imagined. I will just mention one experiment more, in which this was shown in a manner still more striking, and not less conclusive. A small piece of red-hot iron being dropped down into the chamber of a common horse pistol, and the pistol being elevated to an angle of about 45 degrees, upon dropping down into its barrel one of the small globes of powder (of the size of a pea), it took fire, and was projected into the atmosphere by the elastic fluid generated in its own combustion, leaving a very beautiful train of light behind it, and disappearing all at once, like a falling star.

This amusing experiment was repeated very often, and with globes of different sizes. When very small ones were used singly, they were commonly consumed entirely before they came out of the barrel of the pistol; but when several of them were used together, some, if not all of them were commonly projected into the atmosphere on fire.

I shall conclude this paper by some observations on the practical uses and improvements that may probably be derived from these discoveries, respecting the great expansive force of the fluid generated in the combustion of gunpowder.

As the *slowness* of the combustion of gunpowder is undoubtedly the cause which has prevented its enormous and almost incredible force from being discovered, so it is evident, that the readiest way to increase its effects is to contrive matters so as to accelerate its inflammation and combustion. This may be done in various ways, but the most simple and most effectual manner of doing it would, in my opinion, be to set fire to the charge of powder by shooting (through a small opening) the flame of a smaller charge into the midst of it.

I contrived an instrument on this principle for firing cannon three or four years ago, and it was found on repeated trials to be useful, convenient in practice, and not liable to accidents. It likewise supersedes the necessity of using priming, of vent tubes, port-fires, and matches; and on that account I imagined it might be of use in the British navy. Whether it has been found to be so or not I have not yet heard.

Another infallible method of increasing very considerably the effect of gunpowder in fire-arms of all sorts and dimensions, would be to cause the bullet to fit the bore exactly, or without windage, *in that part of the bore at least where the bullet rests on the charge*: for when the bullet does not completely close the opening of the chamber, not only much of the elastic fluid generated in the first moment of the combustion of the charge escapes by the sides of the bullet, but, what is of still greater importance, a considerable part of the unconsumed powder is blown out of the chamber along with it, in a state of actual combustion, and getting before the bullet continues to burn on as it passes through the whole length of the bore, by which the motion of the bullet is much impeded.

The loss of force which arises from this cause is, in some

cases, almost incredible; and it is by no means difficult to contrive matters so as to render it very apparent, and also to prevent it.

If a common horse pistol be fired with a loose ball, and so small a charge of powder that the ball shall not be able to penetrate a deal board so deep as to stick in it when fired against it from the distance of six feet; the same ball, discharged from the same pistol, with the same charge of powder, may be made to pass quite through one deal board, and bury itself in a second placed behind it, merely by preventing the loss of force which arises from what is called windage; as I have found more than once by actual experiment.

I have in my possession a musket, from which, with a common musket charge of powder, I fire two bullets at once with the same velocity that a single bullet is discharged from a musket on the common construction, with the same quantity of powder. And, what renders the experiment still more striking, the diameter of the bore of my musket is exactly the same as that of a common musket, except only in that part of it where it joins the chamber, in which part it is just so much contracted that the bullet which is next to the powder may stick fast in it. I ought to add, that though the bullets are of the common size, and are consequently considerably less in diameter than the bore, means are used which effectually prevent the loss of force by windage; and to this last circumstance it is doubtless owing, in a great measure, that the charge appears to exert so great a force in propelling the bullets.

That the conical form of the lower part of the bore, where it unites with the chamber, has a considerable share in producing this extraordinary effect, is however very certain, as I

have found by experiments made with a view merely to ascertain that fact.

I will finish this paper by a computation, which will show that the force of the elastic fluid generated in the combustion of gunpowder, enormous as it is, may be satisfactorily accounted for upon the supposition that its force depends *solely* on the elasticity of watery vapour, or steam.

It has been shown by a variety of experiments made in England, and in other countries, and lately by a well conducted set of experiments made in France by M. DE BETANCOUR, and published in Paris under the auspices of the Royal Academy of Sciences, in the year 1790, that the elasticity of steam is doubled by every addition of temperature equal to 30 degrees of FAHRENHEIT's thermometer.

Supposing now a cavity of any dimensions (equal in capacity to 1 cubic inch, for instance) to be filled with gunpowder, and that on the combustion of the powder, and in consequence of it, this space is filled with steam (and I shall presently show that the water, existing in the powder *as water*, is abundantly sufficient for generating this steam); if we know the heat communicated to this steam in the combustion of powder, we can compute the elasticity it acquires by being so heated.

Now it is certain that the heat generated in the combustion of gunpowder cannot possibly be less than that of red-hot iron. It is probably much greater, but we will suppose it to be only equal to 1000 degrees of FAHRENHEIT's scale, or something less than iron visibly red-hot in daylight. This is about as much hotter than boiling linseed oil, as boiling linseed oil is hotter than boiling water.

As the elastic force of steam is just equal to the mean pres-

sure of the atmosphere when its temperature is equal to that of boiling water, or to 212° of FAHRENHEIT's thermometer, and as its elasticity is doubled by every addition of temperature equal to 30 degrees of the same scale, with the heat of 212° + 30° = 242° its elasticity will be equal to the pressure of 2 atmospheres; at the temperature of 242° + 30° = 272° it will equal 4 atmospheres;

at 272° + 30° = 302° it will equal 8 atmospheres;
at 302° + 30° = 332° —— 16 ——
at 332° + 30° = 362° —— 32 ——
at 362° + 30° = 392° —— 64 ——
at 392° + 30° = 422° —— 128 ——
at 422° + 30° = 452° —— 256 ——
at 452° + 30° = 482° —— 512 ——
at 482° + 30° = 512° —— 1024 ——
at 512° + 30° = 542° —— 2048 ——
at 542° + 30° = 572° —— 4096 ——

at 572° + 30° = 602°, (or 2 degrees above the heat of boiling linseed oil,) its elasticity will be equal to the pressure of 8192 atmospheres, or above *eight times* greater than the utmost force of the fluid generated in the combustion of gunpowder, according to Mr. ROBINS's computation. But the heat generated in the combustion of gunpowder is much greater than that of 602° of FAHRENHEIT's thermometer, consequently the elasticity of the steam generated from the water contained in the powder must of necessity be much greater than the pressure of 8192 atmospheres.

Following up our computations on the principles assumed, (and they are founded on the most incontrovertible experiments) we shall find that,

at the temperature of 602° + 30° = 632°	} the elasticity will be equal to the pressure of	16,384 atmospheres;
at 632° + 30° = 662°	—	32,768
at 662° + 30° = 692°	—	65,536

and at 692° + 30° = 722°, the elasticity will be equal to the pressure of 131,072 atmospheres, which is 190 times greater than the elastic force assigned by Mr. ROBINS to the fluid generated in the combustion of gunpowder; and about *one sixth* part greater than my experiments indicated it to be.

But even here the heat is still much below that which is most undoubtedly generated in the combustion of gunpowder. The temperature which is indicated by 722° of FAHRENHEIT's scale, (which is only 122 degrees higher than that of boiling quicksilver, or boiling linseed oil,) falls short of the heat of iron which is visibly red-hot in daylight by 533 degrees: but the flame of gunpowder has been found to melt brass, when this metal, in very small particles, has been mixed with the powder; and it is well known that to melt brass a heat is required equal to that of 3807 degrees of FAHRENHEIT's scale; 2738 degrees above the heat of red-hot iron, or 3085 degrees higher than the temperature which gives to steam an elasticity equal to the pressure of 131072 atmospheres.

That the elasticity of steam would actually be increased by heat in the ratio here assumed, can hardly be doubted. It has absolutely been found to increase in this ratio in all the changes of temperature between the point of boiling water (I may even say of freezing water) and that of 280° of FAHRENHEIT's scale; and there does not appear to be any reason why the same law should not hold in higher temperatures.

A doubt might possibly arise with respect to the existence of a sufficient quantity of water in gunpowder, to fill the space in which the powder is fired, with steam, at the moment of the explosion; but this doubt may easily be removed.

The best gunpowder, such as was used in my experiments, is composed of 70 parts (in weight) of nitre, 18 parts of sulphur, and 16 parts of charcoal; hence 100 parts of this powder contain $67\frac{1}{10}$ parts of nitre, $17\frac{1}{10}$ parts of sulphur, and of charcoal $15\frac{4}{10}$ parts.

Mr. KIRWAN has shown that in 100 parts of nitre there are 7 parts of water of crystallization; consequently, in 100 parts of gunpowder, as it contains $67\frac{1}{10}$ parts of nitre, there must be $4\frac{111}{1000}$ parts of water.

Now as 1 cubic inch of gunpowder, when the powder is well shaken together, weighs exactly as much as 1 cubic inch of water at the temperature of 55° F. namely 253.175 grains Troy, a cubic inch of gunpowder in its driest state must contain at least $10\frac{991}{1000}$ grains of water; for it is 100 to 4.711, as 253.175 to 10.927. But besides the water of crystallization which exists in the nitre, there is always a considerable quantity of water in gunpowder, in that state in which it makes bodies *damp* or *moist*. Charcoal exposed to the air has been found to absorb nearly $\frac{1}{4}$ of its weight of water; and by experiments I have made on gunpowder, by ascertaining its loss of weight on being much dried, and its acquiring this lost weight again on being exposed to the air, I have reason to think that the power of the charcoal, which enters into the composition of gunpowder, to absorb water remains unimpaired, and that it actually retains as much water in that state, as it would retain were it not mixed with the nitre and the sulphur.

As there are $15\frac{4}{10}$ parts of charcoal in 100 parts of gunpowder, in 1 cubic inch of gunpowder (= 253.175 grains Troy,) there must be 38.989 grains of charcoal; and if we suppose $\frac{1}{8}$ of the apparent weight of this charcoal to be water, this will give 4.873 grains in weight for the water which exists in the form of *moisture* in 1 cubic inch of gunpowder.

That this estimation is not too high is evident from the following experiment. 1160 grains Troy of apparently dry gunpowder, taken from the middle of a cask, on being exposed 15 minutes in dry air, heated to the temperature of about 200°, was found to have lost 11 grains of its weight. This shews that each cubic inch of this gunpowder actually gave out $2\frac{4}{10}$ grains of water on being exposed to this heat; and there is no doubt but that at the end of the experiment it still retained much more water than it had parted with.

If now we compute the quantity of water which would be sufficient, when reduced to steam under the mean pressure of the atmosphere, to fill a space equal in capacity to 1 cubic inch, we shall find that either that contained in the nitre which enters into the composition of 1 cubic inch of gunpowder as *water of crystallization*, or even that small quantity which exists in the powder in the state of *moisture*, will be much more than sufficient for that purpose.

Though the density of steam has not been determined with that degree of precision that could be wished, yet it is quite certain that it cannot be less than 2000 times rarer than water, when both are at the temperature of 212°. Some have supposed it to be more than 10,000 times rarer than water, and experiments have been made which seem to render this opinion not improbable; but we will take its density at the highest

possible estimation, and suppose it to be only 2000 times rarer than water. As 1 cubic inch of water weighs 253.175 grains, the water contained in 1 cubic inch of steam at the temperature of 212° will be $\frac{1}{2000}$ part of 253.175 grains, or 0.12659 of a grain.

But we have seen that 1 cubic inch of gunpowder contains 10.927 grains of water of crystallization, and 4.873 grains in a state of moisture. Consequently the quantity of water of crystallization in gunpowder is 86 times greater, and the quantity which exists in it in a state of *moisture* is 38 times greater, than that which would be required to form a quantity of steam sufficient to fill completely the space occupied by the powder.

Hence we may venture to conclude, that the quantity of water actually existing in gunpowder is much more than sufficient to generate all the steam that would be necessary to account for the force displayed in the combustion of gunpowder (supposing that force to depend solely on the action of steam), even though no water should be generated in the combustion of the gunpowder. It is even very probable that there is more of it than is wanted, and that the force of gunpowder would be still greater, could the quantity of water it contains be diminished.

From this computation it would appear, that the difficulty is not to account for the force actually exerted by fired gunpowder, but to explain the reason why it does not exert a much greater force. But I shall leave these investigations to those who have more leisure than I now have to prosecute them.

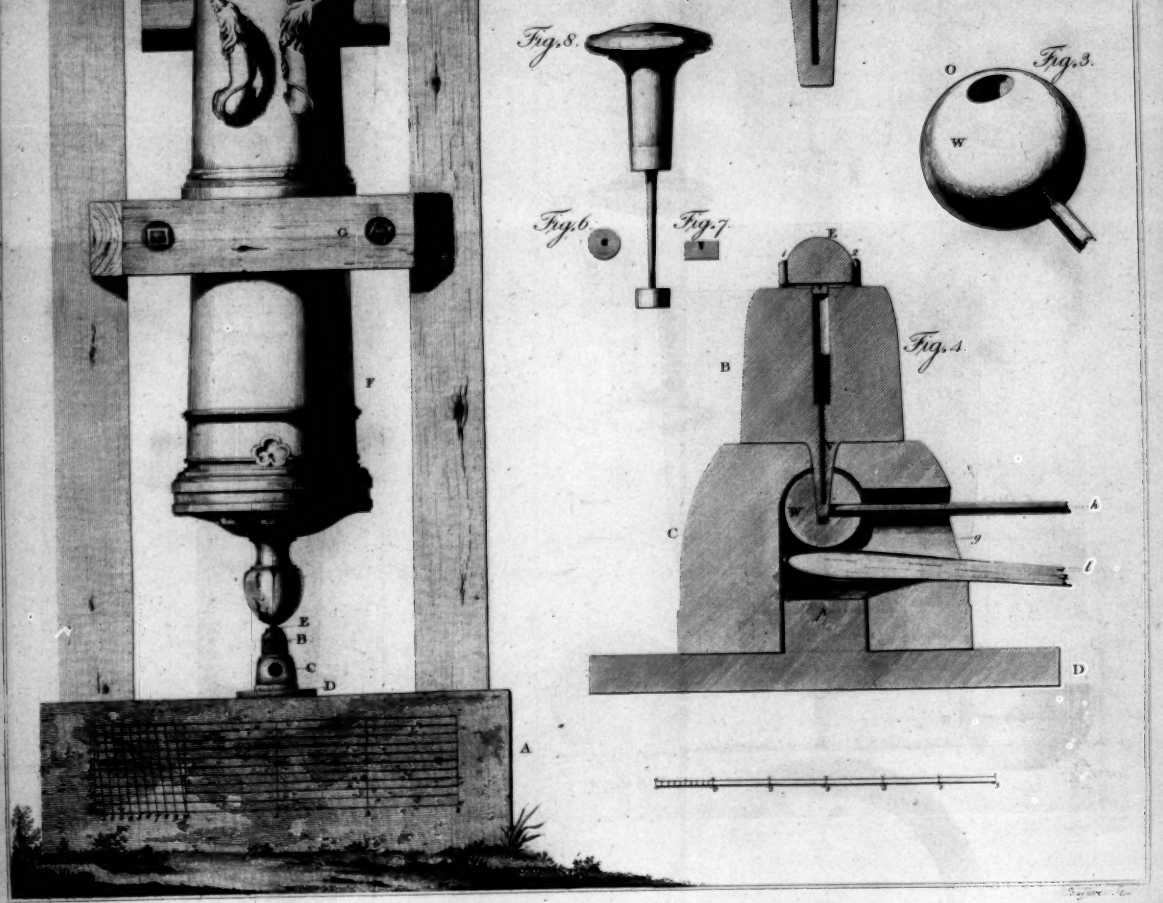
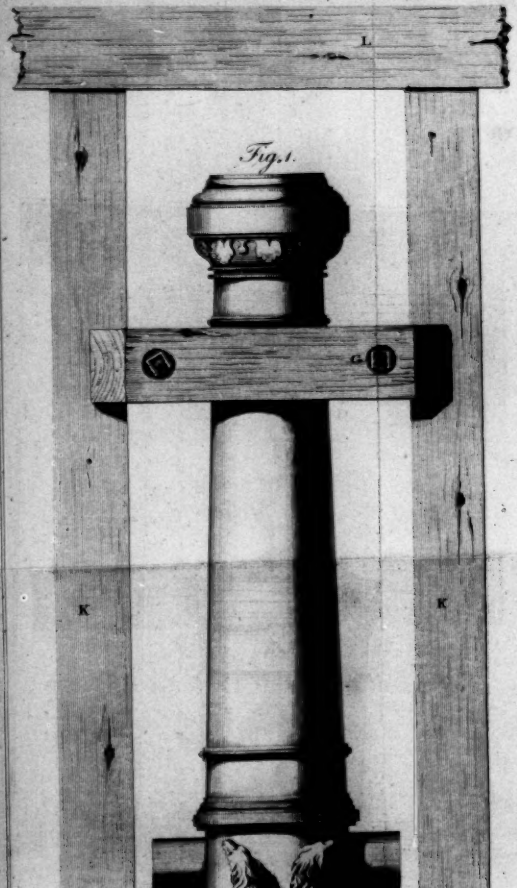


Fig. 8.
Fig. 3.
O
W
Fig. 6.
Fig. 7.
Fig. 4.
E
B
C
D
A
F
G
g
l
m


L
Fig. 1.
G
K
K

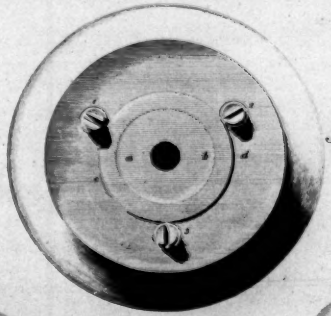


Fig. 5.

Fig. 9.

Fig. 10.
E

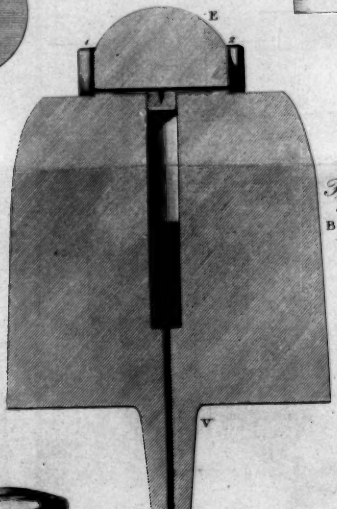
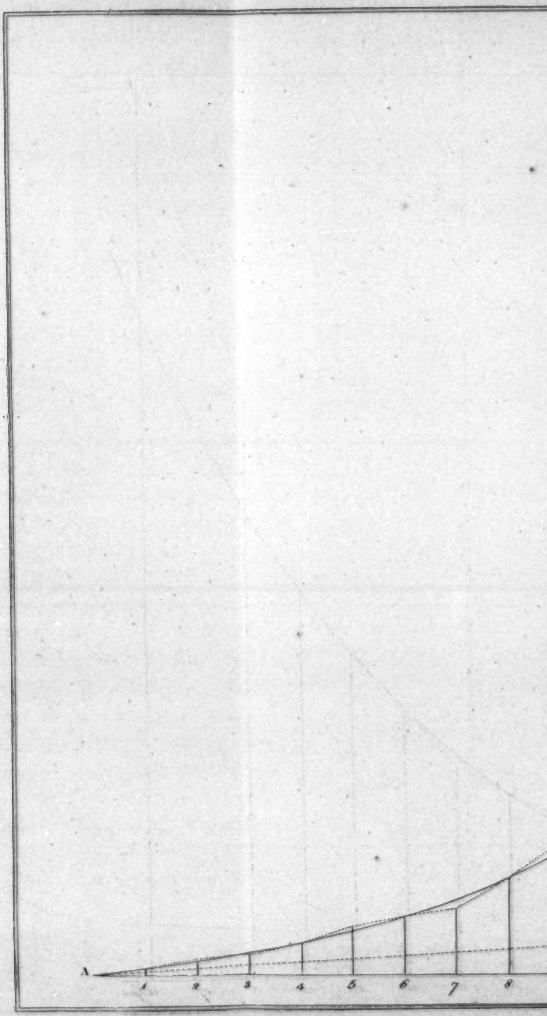
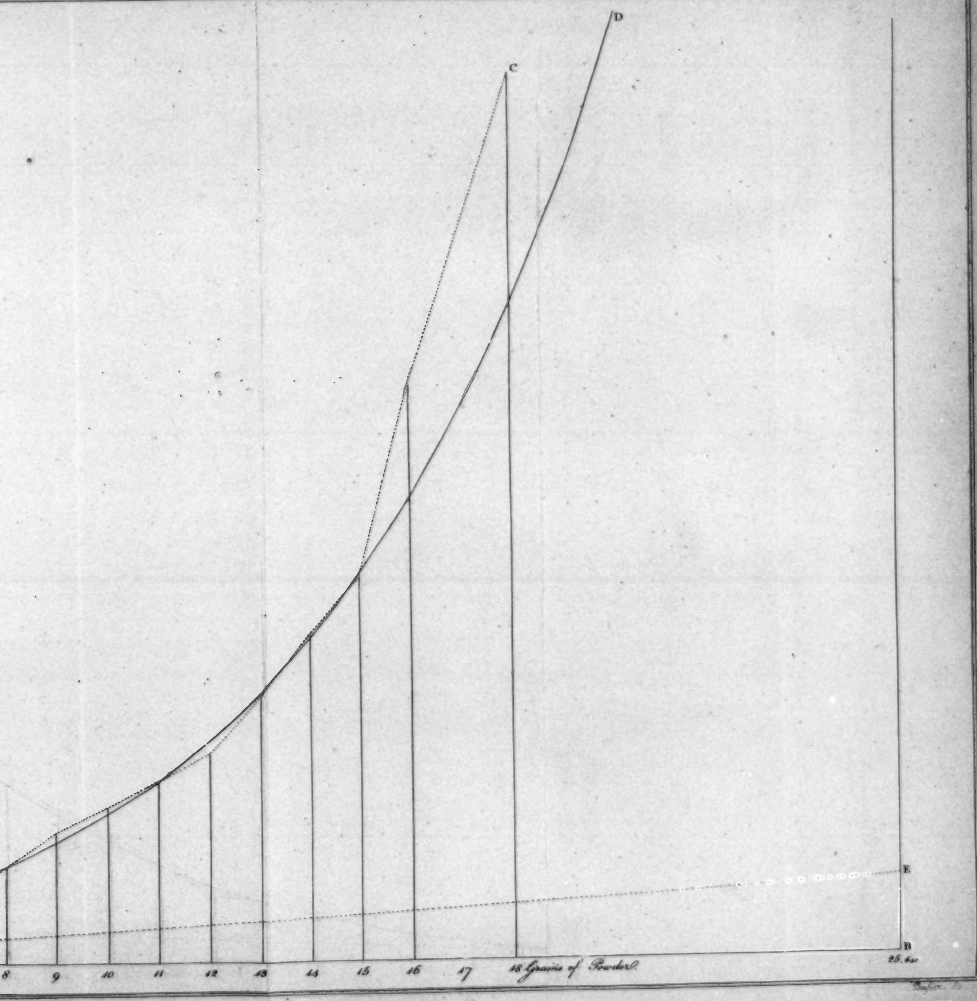

E
Fig. 2.
B
V

Fig. 8.


A
1
2
3
4
5
6
7
8


D
C
E
B
8
9
10
11
12
13
14
15
16
17
18 Grains of Powder.
25.6in

XIII. *A Third Catalogue of the comparative Brightness of the Stars; with an introductory Account of an Index to Mr.* Flamsteed's *Observations of the fixed Stars contained in the second Volume of the* Historia Cœlestis. *To which are added, several useful Results derived from that Index. By* William Herschel, *LL.D. F.R.S.*

Read May 18, 1797.

In my earliest reviews of the heavens, I was much surprised to find many of the stars of the British catalogue missing. Taking it for granted that this catalogue was faultless, I supposed them to be lost. The deviation of many stars from the magnitude assigned to them in that catalogue, for the same reason, I looked upon as changes in the lustre of the stars. Soon after, however, I perceived that these conclusions had been premature, and wished it were possible to find some method that might serve to direct us from the stars in the British catalogue, to the original observations which have served as a foundation to it. The labour and time required for making a proper index, withheld me continually from undertaking the construction of it: but when I began to put the method of comparative brightness in practice, with a view to form a general catalogue, I found the indispensable necessity of having this index recur so forcibly, that I recommended it to my Sister to undertake the arduous task. At my request, and according

to a plan which I laid down, she began the work about twenty months ago, and has lately finished it.

The index has been made in the following manner. Every observation upon the fixed stars contained in the second volume of the *Historia Cœlestis* was examined first, by casting up again all the numbers of the screws, in order to detect any error that might have been committed in reading off the zenith-distance by diagonal lines. The result of the computation being then corrected by the quantity given at the head of the column, and refraction being allowed for, was next compared with the column of the correct zenith-distance as a check.

Every star was now computed by a known preceding or following star; and its place according to the result of the computation laid down in the *Atlas Cœlestis*, by means of proportional compasses. This was necessary, in order to ascertain the observed star: for the observations contain but little information on the subject; most of the small stars being without names, letters, or descriptions. The many errors in the names of the constellations affixed to the stars, and in the letters by which they are denoted, also demanded a more scrupulous attention; so that only their relative situation, examined by calculation, could ascertain what the stars really were which had been observed.

Every observed star being now ascertained, its number in the British catalogue was added in the margin at the end of the line of the observation; and a book with all the constellations and number of the stars of the same catalogue, with large blank spaces to each of them, being provided, an entry of the page where FLAMSTEED's observation is to be found, was made in its proper place.

If the star observed was not in the British catalogue, it was marked as such in the margin of the observations; and being provided with another book of constellations and numbers, it was entered into the blank space belonging to some known preceding or following star, by which its place had been settled. The Greek and English letters used by FLAMSTEED, whether they were such as had been introduced before, or which he thought it expedient to add to them at the time of observation, were also entered into their proper places; and to complete the whole, the magnitude affixed to the stars was likewise joined to the entry made in the blank spaces of the index.

I have been so far particular in giving the method by which the index has been constructed, that it may appear what confidence ought to be given to the conclusions which will be drawn from its report.

About three or four examples of its use, will completely shew how the results, which will be mentioned, have been obtained.

Suppose I wish to be informed of the particulars relating to the 13th Arietis. Then by the index I am referred, in the column allotted for that star, to 77 observations; and find that FLAMSTEED used the letter *a* 72 times, and that in two places he calls it a star of the 2d magnitude; the rest of the observations being without any estimation of its brightness.

If it be required to know FLAMSTEED's observations upon the 34th Tauri, which star is supposed to have been the Georgian planet, mistaken by FLAMSTEED for a small fixed star;* we find in our index, that on page 86, December 13, 1690, a star of the 6th magnitude was observed, which answers to the

* See *Astronomisches Jahr-Buch* for 1789, page 202.

place of the 34th Tauri in the British catalogue; and that no other observation of the same star occurs in the second volume. In my catalogue of comparative brightness, the 34th Tauri is put down among the lost stars, it being no longer to be seen in the place where it was observed by FLAMSTEED.

If in my review of the heavens I cannot find 38 Leonis, and examine this index, I am at once informed that FLAMSTEED never observed such a star; and that of consequence it has been inserted in the British catalogue by some mistake or other. In many cases, these mistakes may be easily traced, as has been shewn with regard to this star in my second catalogue of comparative brightness. See the note to 38 Leonis.

When we wish to examine 90 Ceti in the heavens, and cannot find it, we are informed by our index, that 90 Ceti is the same star with 1 Eridani; and that, consequently, we are not to look out for two different stars.

We may now proceed to give some general results that are to be obtained from an inspection of our index. They are as follows.

111 Stars inserted in the British catalogue have never been observed by FLAMSTEED. This will explain why so many stars in the heavens seem to have been lost.

There are 39 stars in the same catalogue that want considerable corrections in right-ascension or polar-distance. In many it amounts to several degrees.

54 stars more, besides the 39 that are taken from the erroneous stars in the catalogue, want corrections in the *Atlas Cœlestis*; several of them also of many degrees.

42 stars are put down, which must be reduced to 21; each going by two names in different constellations.

371 stars, completely observed both in right-ascension and zenith-distance, have been totally overlooked.

35 more, which have one of the two, either right-ascension or polar-distance doubtful, have been omitted.

86 with only the polar-distance, and 13 with only the right-ascension, have also been unnoticed.

About 50 more that are pointed out by pretty clear descriptions, are likewise neglected; so that upon the whole between five and six hundred stars observed by FLAMSTEED, have been overlooked when the British catalogue was framed.

These additional stars will make a considerable catalogue, which is already drawn up and nearly finished by Miss HERSCHEL, who is in hopes that it may prove a valuable acquisition to astronomers.

Neither the index to FLAMSTEED's observations, nor the catalogue of omitted stars, were finished when my former two catalogues of comparative brightness were given; I shall therefore now select a few notes to be added to those which are at the end of these catalogues. They will contain such additional light as I have been enabled to gather from this newly acquired assistance.

Additional Notes to the Stars in the First Catalogue of the comparative Brightness of the Stars.

Aquarius.

25 Is the same star with 6 Pegasi. There are but two observations upon it. The first is on page 57; FLAMSTEED calls it "*in constellatione Pegasi sub capite.*" The second, on page

71, is described "*in constellatione Aquarii trianguli in capite "præcedens et borealis.*" Here we see that the double insertion in the catalogue is owing to the star's having been called by different names in the observations. See also Mr. WOLLASTON's catalogue, zone 88°.

27 Is the same with 11 Pegasi. There are three observations: the first places the star in the constellation of Pegasus, the two latter in that of Aquarius. See also Mr. WOLLASTON's catalogue for this star, and others of the same kind.

65 Has not been observed by FLAMSTEED; notwithstanding which we find it inserted in my first catalogue, where its relative brightness is given. It should be considered that, in the first place, several stars of which there are no observations in the second volume of FLAMSTEED's works, and which are, nevertheless, inserted in the British catalogue, such for instance as θ and ι Draconis, are well known to exist in the heavens. Now whether they were put into the catalogue from observations that are not in the second volume, or taken from other catalogues, it so happens that observations of them cannot be found. Therefore the want of a former observation by FLAMSTEED, is not sufficient to prove that a star does not exist. In the next place it should be recollected, that the method used to ascertain the stars in estimating their brightness, is not so accurate, as to point out with great precision the absolute situation of a star; and that, consequently, another star which happens to be not far from the place where the catalogue points out the star we look for, may be taken for it; especially when there are no neighbouring stars of the British catalogue that may induce us to exert uncommon attention in ascertaining the identity of such a star. MAYER, however, has an obser-

vation of 65 Aquarii in his zodiacal catalogue, No. 932, which puts the existence of the star out of doubt.

72 As the star neither was observed by FLAMSTEED, nor does exist, we cannot admit the remark which Mr. WOLLASTON in his catalogue, zone 95°, has upon MAYER's 939 star; where he supposes an error in declination of 3 degrees to have been committed, on a supposition of its being FLAMSTEED's 72.

80 Requires + 2′ in time in RA, and therefore is not the star I have given, which requires — 1′ 35″.

104 Which is without RA in the British catalogue, has three complete observations, page 8, 70, and 331.

Aquila.

29 Is without RA. There is but one observation of FLAMSTEED, page 53, which has no time. The RA is given by M. DE LA LANDE, in Mr. BODE's *Jahr-Buch* for 1796, page 163.

33 and 34 Which do not exist, were probably inserted by a mistake of one hour in the time of one of the observations on the two stars 68 and 69. In the zenith-distance, page 71 of FLAMSTEED's observation of 69 Aquilæ, for 53° read 55°.

40 and 43 Which do not exist, were probably also inserted by the same mistake of one hour in the RA of 70 and 71.

Capricornus.

1 and 2 Should be ξ^1 ξ^2. FLAMSTEED calls them so in his observations, and MAYER has also adopted the same letters in his catalogue, No. 821 and 822.

Cygnus.

5 Is without RA in the British catalogue; but the star has not been observed by FLAMSTEED.

9 Is without RA; FLAMSTEED, however, has a complete observation of it, page 67.

24 Has no RA. The time observed by FLAMSTEED is only doubtful in the seconds. Its RA has been given in Mr. BODE's *Jahr-Buch* for 1797, page 163.

33 Has no RA. FLAMSTEED never observed this star; but it is 3 Cephei Hevelii.

38 Has no RA in the British catalogue; but as the defective and only observation of FLAMSTEED on page 75, which might be supposed to belong to 38, will agree better with 43, it follows that he never observed 38.

68 Has no RA. There is a complete observation by FLAMSTEED, page 75.

78 Has no time in FLAMSTEED's observations. It is No. 146 in DE LA CAILLE's catalogue.

79 Has no RA. FLAMSTEED has but one observation, which is without time. Mr. BODE gives it in his *Jahr-Buch* for 1797, page 163.

Hercules.

24 Is the same with 51 Serpentis.

28 Is the same with 11 Ophiuchi.

54 There is no observation of this star. The zenith-distance of 55 was taken twice April 8, 1703 (instances of which we find in several other stars), which occasioned its being inserted as two stars.

63 There is no observation of this star, nor does it exist. The star of which the brightness is given in my catalogue, is at some distance from the place assigned in the British catalogue. FLAMSTEED observed a star, page 444, which will be No. 269 in Miss HERSCHEL's manuscript catalogue. This, with an error in the calculation of the PD, probably occasioned the insertion of 63. And if this be the star, the PD of the British catalogue must be corrected + 3°.

71 Has never been observed by FLAMSTEED, nor does it exist. A small error in the calculation of one of the four observations of 70, may have produced it.

80 and 81 Were never observed. The two stars ν 24 and 25 Draconis, miscalled ι in FLAMSTEED's observations, page 55 and 175, with an error of PD, accounts for the insertion of these stars. See Mr. BODE's *Jahr-Buch* for 1787, page 194.

93 The PD is marked : : (doubtful), in the British catalogue; but the observation of FLAMSTEED, page 520, is complete.

Pegasus.

6 Is the same star with 25 Aquarii.

11 Is the same star with 27 Aquarii.

Additional Notes to the Stars in the Second Catalogue of the comparative Brightness of the Stars.

Aries.

1 There is an observation of a star by FLAMSTEED, which being calculated with an error of 10′ of time in RA, would produce 1 Arietis; we may therefore correct the British cata-

logue RA + 10′, and the star will be found to exist. In Miss HERSCHEL'S manuscript catalogue it is No. 143.

2 Is the same star with 107 Piscium.

38 is the same star with 88 Ceti. In three observations, page 85, 285, and 485, FLAMSTEED has called it Arietis; and on page 481 he has called it Ceti. See also Mr. BODE'S *Jahr-Buch* for 1793, page 200.

50 By FLAMSTEED'S observation, page 273, the catalogue requires — 1′ in time of RA.

Cassiopea.

3 The place in the catalogue by two observations of FLAMSTEED requires + 5′¼ of time in RA, and + 7′ of PD.

8 Is marked : : but has four complete observations on page 140, 144, 145, and 147.

29 There is an observation of FLAMSTEED on page 144 which has produced this star, but the time of it requires a correction of + 6′; and it will then belong to 32. That this correction should be used, will appear when we compare this observation with another on page 213. In both places a star which is not inserted in the British catalogue, but which is No. 384 of Miss HERSCHEL'S manuscript catalogue, was taken at the same time. On page 144 it is "Duarum "infra γ, versus polum, borealis. Simul fere transit, austrea;" and on page 213 we have "post transitum" for the new star, and "cum priore" for 32; and in both places the zenith-distance perfectly shews that they were the same stars: the 32d and a star south of it. And they are now both in the places where FLAMSTEED has observed them.

30 FLAMSTEED has no observation of this star. It is μ 21 Cassiopeæ Hevelii.

33 FLAMSTEED observed no RA of this star. It is θ 23 Cassiopeæ Hevelii.

34 Is wrong in the catalogue. By two observations of FLAMSTEED, page 144 and 521, it requires a mean correction of — 9′ of time in RA. In this case my double star III. 23 will no longer be φ 34 Cassiopeæ, but a star 9′ of time preceding φ; for it exists in the place where 34 is put in Atlas, according to the erroneous catalogue, and is rather larger than FLAMSTEED's star φ.

35 The RA is marked : : The single observation, page 207, has the time marked *circiter*, being probably set down to the nearest minute only; and by the same observation the PD requires + 20′.

47 Is also marked : : but has one complete observation, page 149.

51 The observation of FLAMSTEED which produced this star should be corrected + 1 hour. This makes it 87 Cassiopeæ Hevelii.

52 and 53 By FLAMSTEED's observation page 208, should be the reverse in PD of what they are.

Cetus.

14 If we correct the British catalogue + 3° in PD, it will become a star observed by FLAMSTEED, which is No. 312 in Miss HERSCHEL's manuscript catalogue.

26 FLAMSTEED has no observation of this star; but we find it in DE LA CAILLE's zodiacal catalogue, No. 10.

51 Is the same with 106 Piscium. FLAMSTEED has 23 ob-

servations of the star, and has always called it ν, except once on page 482, where it is without letter, and where the constellation is marked Aquarii: now, as there was immediately following an observation of 54 Ceti, and Aquarius was evidently wrong, the star has been put in Cetus.

58 By FLAMSTEED's observation, page 358, the RA in the British catalogue requires a correction of — 3′ in time.

74 FLAMSTEED has no observation of this star, nor can I find it in any other catalogue. The place of it is so distant from other stars of the British catalogue, that my estimation of brightness may belong to some star not far from the situation assigned, and that the star of the British catalogue may not exist.

88 Is the same with 38 Arietis. See Mr. BODE's *Jahr-Buch* for 1793, page 200.

Eridanus.

44 In the British catalogue is marked :: The single observation of FLAMSTEED, page 153, is perfect, all but a difference of 5′ between the zenith-distance by the diagonal lines and by the screw.

45 Marked :: has a complete observation, page 153.

68 Marked :: has a complete observation, page 146.

Gemini.

50 There is no observation on this star. The star I have given is at a considerable distance from the place assigned by the British catalogue, so that in fact the star of the catalogue does not exist. It has been inserted in the British catalogue by a mistake in the calculation of a star which is about 1° 49′ more

south. This will be No. 139 in Miss HERSCHEL's manuscript catalogue, and it is probably the real intended 50 of FLAMSTEED. The expression of its brightness 41,50 of my catalogue will do very well for it.

70 and 71 By FLAMSTEED's observations should be called π^1, and π^2. TYCHO and HEVELIUS also call 71 π.

72 and 73 Have been inserted by a mistake in 64 and 65. See Mr. BODE's *Jahr-Buch* for 1788, page 175.

76 FLAMSTEED has no observation of this star. It is, however, MAYER's No. 310.

80 Is not π, but according to FLAMSTEED's observation *quæ sequitur* π; and has no letter.

Leo.

10 Is the same with 1 Sextantis.

25 This star does not exist in the place where the British catalogue gives it; but if we admit that it has been inserted by a mistake in the calculation of 10 Sextantis, it may be taken into the constellation of Leo, as a star inserted in two constellations; and it will then be "25 is the same with 10 "Sextantis."

26 In FLAMSTEED's observations, page 299, the *strias cochleæ* give 26′ less than the *lineas diagonales*. The former are right; therefore the British catalogue must be corrected PD — 26′.

28 FLAMSTEED has no observation of this star. It was probably inserted by a mistake in calculating an imperfect observation of 11 Sextantis. If this be allowed, we then must say "28 is the same with 11 Sextantis."

66 FLAMSTEED has no observation of this star. There is

a small star near the place where the British catalogue has given it, of which I have expressed the brightness; but as its situation is not exactly where it ought to be, my catalogue should have, "does not exist."

67 Is the same with 53 Leonis minoris.

71 May have been inserted by a mistake in one of the three observations of 73; putting the star north of θ instead of south.

III. *Catalogue of the comparative Brightness of the Stars.*

			Lustre of the stars in Andromeda.
1	ο	3.4	15 – 1 – 16
2		6	20 – 2, 4
3		6	8. 3
4		6	2, 4, 6
5		6	11 ⁊ 5
6		6.7	4, 6
7		5.6	7 – 8
8		6	7 – 8, 11 8. 3
9		6	10. 9
10		6.7	13 – 10. 9
11		6	8, 11 ⁊ 5
12		6	15. 12, 13
13		6	12, 13 – 10
14		6	14, 15
15		6	14, 15. 12
16	λ	4	16 – 17 1 – 16
17	ι	4	16 – 17, 19 19 ⁊ 17
18		6	20. 18
19	κ	4	17, 19 – 20 19 ⁊ 17
20	ψ	5.6	19 – 20 20 – 2 22 – 20. 18 22 – 20 – 23
21	α	2	21, 48 21 ⁊ 8 Pegasi 21 ⁊ 48 21 – 48
22		5	22 – 20
23		6	20 – 23, 26
24	θ	4.5	25, 24 – 27
25	σ	5	25, 24
26		6	23, 26
27	ϱ	5	24 – 27
28		6	29 – 28 32. 28 ⁊ 40

Lustre of the stars in Andromeda.			
29	π	4.5	30 . 29 – 28 29 , 35
30	ϵ	4	37 – 30 . 29
31	δ	3	4 Trianguli = 31 –, 2 Trianguli
32		6	35 – 32 . 28 32 – 39
33		Neb.	is a Nebula
34	ζ	4	35 , 34 , 38
35	ν	4	29 , 35 – 32 35 , 34 35 – 48 50 –, 35 , 53
36		6	38 –, 36
37	μ	4.3	37 – 30 37 – 50
38	η	4.5	34 , 38 –, 36
39		6	32 – 39
40		6	28 ; 40
41	d	5	42 –, 41 . 45
42	φ	5	54 ; 42 –, 41
43	β	2	21 , 43 . 57 21 ; 43 ; 57 21 – 43 , 57 43 ; 13 Ari . 43 – 13 Ari 43 ; 57
44		6	45 . 44
45		5.6	41 . 45 . 44 45 . 47
46		4.5	48 , 46 , 49
47		6	45 . 47
48		5	35 – 48 , 46
49	ξ	5	46 , 49
50	υ	6.5	37 – 50 –, 35
51	υ	5	51 – 1
52	λ	6	53 , 52 . 55
53	τ	5	35 , 53 , 52 58 , 53 – 56 53 , 60
54	φ	4	54 ; 42
55		Neb.	52 . 55
56		6	53 – 56 . 59 60 , 56

Lustre of the stars in Andromeda.			
57	γ	2.3	43.57 57; 18 Arietis 43; 57 43, 57 43 ; 57
58		6	58, 53
59		6	56.59
60	b	6	53, 60, 56
61		6	63, 61 66, 61
62	c	6	65, 62
63		6	64.63, 61 6 Persei, 63
64		6	65-64.63
65		5	65-64 65, 62 65, 6 Persei
66		6.7	66; 61
Lustre of the stars in Bootes.			
1		6	7, 1 6-1, 2
2		6	1, 2.10
3		6	11.3
4	τ	4	5 ; 4-6
5	v	4	5 ; 4 30-, 5, 35
6		5.6	6, 7 4-6-1
7		7	6, 7, 1 7-26
8	η	3	8, 27 79 Virginis; 8 8-27 36-8
9		5	12 ; 9-11
10	e	7	2.10
11		7.6	9-11.3
12	d	5	28; 12 ; 9
13		6	13-24
14		6	18, 14, 15
15		6	14, 15
16	α	1	16--3 Lyræ
17	$\varkappa$	4	21.17

Lustre of the stars in Bootes.			
18		6	20, 18, 14
19	λ	4	19 · 23
20		5	20, 18 20; 22
21	ι	4	23, 21. 17
22	f	5	20; 22
23	θ	4	19 · 23, 21
24	g	6.7	13 – 24
25	ϱ	4	25 – – 51
26		7	7 – 26 34 – – 26
27	γ	3	8, 27 – 49 27 –, 49 8 – 27 27 –, 42
28	σ	5	51 – 28 28; 12
29	π	4.3	35, 29
30	ζ	3	30 –, 5
31		5	35 – 31 – 32
32		6	31 – 32
33	b'	6	39 · 33 – 38
34		6	34 – – 26
35	o	4.5	5, 35, 29 37 · 35 – 31
36	s	3	5 Coronæ – 36 – 8
37	ξ	4	37 · 35
38	b'	6	33 – 38
39		6	47 · 39 39 · 33
40		6.7	47 – 40
41	ω	5	45; 41 – 46 41, 48 41 · 50
42	β	3	49, 42 42 –, 49 27 –, 42 · 49 42 ‡ 49
43	ψ	5	48 – 45
44		6	44, 47
45	c	5	48 – 45; 41
46	b	6	41 – 46 48, 46
47	k	5	44, 47 · 39 47 – 40
48	χ	5	41, 48, 46

Lustre of the stars in Bootes.			
49	δ	3	27 –, 49 42, 49 42 . 49 42 , 49 42 ⁞ 49 27 – – 49
50		5	41 . 50
51	μ	4	25 – – 51 – 28 4 Coronæ . 51 , 7 Coronæ
52	ν^1	6	53 ; 52 , 54
53	ν^2	6	53 ; 52
54	ϕ	6	52 , 54

Lustre of the stars in Cancer.			
1		6	5 . 1
2	ω^1	6	9 , 2 , 4 14 , 2 , 4
3		6	16 –, 3 , 5 8 – 3 , 12
4	ω^2	6	2 , 4 . 13
5		6	3 , 5 . 1
6	χ	5	6 – 14 6 – 15 6 ⁝ 18
7		8	9 , 7
8		6	8 – 3
9	μ^1	7	10 – 9 , 2 9 , 7
10	μ^2	5	10 – 9
11		6	14 , 11 15 – 11
12		6	3 , 12
13	ψ^1	6 . 7	4 . 13
14	ψ^2	4	14 , 2 6 – 14 , 11
15	ψ^3	5	6 – 15 – 11
16	ζ	5 . 6	43 . 16 –, 3
17	β	4 . 3	17 ⁝ 47 17 , 48
18	χ	6	6 ⁝ 18 ; 23
19	λ	6	19 – 30 , 28
20	d^2	6	31 , 20 , 25
21		6	37 , 21 , 34 29 – 21

Lustre of the stars in Cancer.			
22	φ^1	6.7	23, 22
23	φ^2	6	18, 23, 22
24	υ^4	6	32 . 24
25	d^2	6	20, 25
26	φ^3	6	Does not exist.
27		6	27; 29
28	υ^3	6.7	30, 28, 32
29		6.7	27; 29 – 21
30	υ^1	6	19 – 30, 28
31	θ	6.5	31, 20 31 . 33
32	υ^2	7.8	28, 32 . 24
33	η	6.7	31 . 38
34		6	21, 34 . 36
35		7	42; 35 . 38
36	c^1	6	34 . 36
37	c^2	6	49 – 37, 21
38	o	8	42; 38 . 40 35 . 38
39		6	39, 41
40		6	38 . 40
41	s	7	39, 41 . 42
42	c	7.8	41 . 42; 38 42; 35
43	γ	4	43 . 16 47 –, 43
44		6	20 – – – 44 4 – – 44
45	A^1	6	76, 45 . 60
46		6	55; 46 . 61
47	δ	4	17; 47 –, 43 65 . 47 – – 76 48, 47
48	i^1	5	17, 48, 47 48 – – 58
49	b	6	49 – 37
50	A^2	6	60, 50
51	σ^1	6	51 . 64
52		6	54 – 52

Lustre of the stars in Cancer.			
53	ξ^2	6	55 ; 53
54		7	62 –, 54 – 52 82 , 54 , 81
55	ξ^2	6	58 – 55 ; 53 67 . 55 , 70 57 – 55 ; 46
56	ξ^1	6	Does not exist.
57	ι^1	5.6	58 ; 57 – 55 57 , 72
58	ξ^3	6	48 – – 58 – 55 58 , 75 58 ; 57
59	σ^2	5.6	64 . 59 . 66
60	α^2	4.5	45 . 60 , 50
61		6	46 . 61
62	o^1	6	63 . 62 –, 54
63	o^2	6	63 . 62
64	σ^1	6	51 . 64 . 59
65	α^1	4	65 . 47
66	σ^3	6	59 . 66
67	ξ^4	6.7	67 . 55
68		6	81 – 68 , 71 68 . 78 68 ; 80
69	ν	6	69 ; 77
70	ξ^5	6.7	55 , 70
71		7	68 , 71 78 , 71
72	τ	6.7	57 , 72
73		6	Does not exist.
74		6	Does not exist.
75		6.7	58 , 75
76	$\varkappa$	4.5	47 – – 76 , 45
77	ξ	5.6	69 ; 77 –, 79
78		6	68 . 78 , 71 83 , 78 80 ; 78 , 71
79		8	77 –, 79
80		7	80 – 83 68 ; 80 ; 78
81	π	7	54 , 81 81 – 68 81 , 83
82		6	82 , 54
83		6	81 , 83 80 – 83 , 78

		Lustre of the stars in Centaurus.	
1	*i*	4.5	3 . 1 . 5
2	*g*	4.5	5 – 2
3	*k*	4.5	4 , 3 . 1
4	*b*	4.5	4 , 3
5	*θ*	2.3	1 . 5 – 2
		Lustre of the stars in Cepheus.	
1	*κ*	5	1 . 17
2	*θ*	5	3 –, 2
3	*η*	4	32 , 3 –, 2 21 $\frac{1}{2}$ 3 32 $\frac{1}{2}$ 3
4		6	6 – 4 . 7
5	*α*	3	5 –, 37 Cygni 5 . 37 Cassiopeæ
6		6	6 – 4
7		6	4 . 7
8	*β*	3	35 –, 8 –, 32
9		6	17 – 9 – 12 11 , 9
10		5	10 . 17
11		5	11 , 9
12		7	9 – 12
13	*μ*	6	13 , 14
14		6	13 . 14 –, 15
15	*ν*	7.6	14 –, 15 – 15
16		5.6	24 , 16 , 78 Draconis
17	*ξ*	5	1 . 17 , 38 10 . 17 – 9 28 , 17 – – 30
18			19 , 18 ; 20
19		6	22 . 19 , 18
20		6	18 ; 20
21	*ζ*	4.5	21 $\frac{1}{2}$ 3
22	*λ*	6	22 . 19
23	*ι*	4	23 , 17
24		5.6	24 , 16

Lustre of the stars in Cepheus.			
25		7	26–, 25
26		6	30–, 26–, 25
27	δ	4.5	32, 27 27.23 21–27–23 21 = 7 27
28		6	28–29
29	ε	6	28–29
30		6	17––30–, 26
31		6	34–31
32	ι	4	8–, 32.3 32 ∤ 3
33	π	5	17, 33
34	ο	5	34–31
35	γ	3	35–, 8
Lustre of the stars in Corona Borealis.			
1	ο	6	2––1
2	η	5	2––1
3	β	4	8; 3–13
4	θ	4.5	13; 4–10 4, 7
5	α	2.3	55 Ophiuchi, 5 5–36 Bootis
6	μ	5	11–6, 9
7	ζ	4	4, 7, 10
8	γ	4	8; 3
9	π	5	6, 9 12, 9
10	δ	4	4–10 7, 10
11	κ	5	11–6
12	λ	5	12, 9
13	ε	4.5	3–13; 4
14	ι	5.6	14, 19
15	ρ	6	17, 15
16	τ	6	16–17
17	σ	6	16–17 17, 15
18	υ	6	19–, 18

Lustre of the stars in Corona Borealis.			
19	ξ	5	14, 19-, 18
20	ν'	5	20 =' 21
21	ν²	5	20 =' 21
Lustre of the stars in Lacerta.			
1		5	7, 1, 8 1, 1 Hevelii . 6
2		5	7, 2, 5
3		4·5	4·3, 9
4		5	5·4·3
5		4·5	7-5 2, 5·4
6		5	7-6, 11 1 Hevelii . 6
7		4	7-5 7, 2 7-6 7, 1
8		6	1, 8. 10
9		6	3, 9
10		6	8. 10, 12
11		5	6, 11, 15
12		6	10, 12
13		6	13. 13, 14
14		6	13, 14. 16
15		5	11, 15. 13
16		6	14. 16
Lustre of the stars in Lepus.			
1		9	7-1 10. 1. 12
2	ι	4	5, 2, 13
3	ι	5	3. 6
4	κ	5	6, 4, 7 4, 8
5	μ	4	9, 5, 2 5, 14
6	λ	4·5	3. 6, 4
7	ν	5·6	4, 7 8; 7-1
8		6	4, 8; 7

Lustre of the stars in Lepus.			
9	β	3	11 – 9 , 5
10		6	10 . 1
11	α	3	11 – 9
12		6	1 . 12
13	γ	3 . 4	2 , 13 , 15
14	ζ	4	5 , 14 , 16
15	δ	4 . 3	13 . 15
16	η	4	14 , 16 – 18
17		6	18 , 17 –, 19
18	θ	4	16 – 18 , 17
19		6	17 –, 19
Lustre of the stars in Navis.			
1		6	686 De la Caille – 1 – 12
2		6	5 . 2 . 10
3	τ	4 . 5	3 , 11
4		6	4 , 9 4 , 6
5		6	9 . 5 . 2
6		5	4 , 6 . 9
7	ξ	3 . 4	15 – – 7 = ♉ 11
8		5 . 6	10 , 8
9		4	4 , 9 . 5 6 . 9
10		6	2 . 10 , 8
11	ε	4	7 = ♉ 11 – 12 11 . 16 11 –, 12 8 , 11 ♉ 665 De la Caille.
12		6	11 – 12 11 –, 12 – 1
13		4	13 , 13 Canis min. 13 – 13 Canis min.
14		6	16 – 14 16 –, 14
15	ι	3	15 , 81 Canis maj. 15 – – 7
16		5	11 . 16 – 14 16 –, 14
17		6	20 – 17 –, 18

			Lustre of the stars in Navis.
18		6	20, 18–, 22
19		4.5	19, 20
20		5.6	19, 20–21 19, 20, 18 20–, 21 20–17
21		6	20–21 20–, 21
22		6	18–, 22
			Lustre of the stars in Orion.
1		4	1–, 3· 1–8
2	π^1	4	3––2–7
3		4	1–, 3 8, 3––2 3, 9
4	o^1	4.5	9–4 11, 4, 15 4–– 96 Tauri 4, 97 Tauri.
5		6	10–, 5
6	g	6	7–6, 14
7	π^2	6	2–7–6
8	Z	4	1–8, 3 8–, 10
9	o^2	4.5	3–9–4
10		4.5	8–, 10–, 5
11	y^1	5	11, 4
12		6	Does not exist.
13		6	16–13 18–13
14	i	5	6, 14; 16
15	y^2	5	4, 15–35
16	b	6	14; 16–13 16. 18
17	ρ^1	4.5	25, 17–21
18		6.5	16. 18–13
19	β	1	19 = ; 10 Canis min. 19 = ; 87 Tauri 19–, 10 Canis min.
20	τ	4	20, 29 28–20 = 29
21		6	17–21
22		5	22, 27 22. 31 22–11 Monocerotis

Lustre of the stars in Orion.			
23	*m*	6	30 ; 23, 38
24	γ	2	112 Tauri -, 24 - 46 24 -, 46
25	ψ^3	5	25, 17 47, 25 . 30
26		6	Does not exist.
27	ρ^2	6	22, 27 31, 27
28	π	3	44 - 28, 48 28 - 20
29	*e*	5	20; 29, 36 20 = 29; 53
30	ψ^3	5	25 . 30 ; 23
31		6	22 . 31, 27
32	A	5	32, 47
33	*n*	6	38, 33
34	δ	2	50 - 34; 53 50 -, 34 53 ; 34
35		6	15 - 35
36	*v*	4	29, 36 - 49
37	ϕ^1	5	40 - 37 61, 37
38		6	23, 38, 33
39	λ	4	39 - 40
40	ϕ^2	5	39 - 40 - 37 40, 61
41	θ^1	6	41 . 43
42	c^1	5	42, 45
43	θ^2	4	41 . 43
44	*i*	3.4	44 - 28
45	c^2	5	42, 45
46	*s*	2	46, 50 - 34 24 - 46 46 - 30 Hydræ 46 - 50 24 -, 46
47	ω	5	32, 47, 25
48	σ	4	28, 48
49	*d*	5	36 - 49 49 - 55
50	ζ	2	50, 24 Gemin 46, 50 - 34 46 - 50 -, 34 50 - 24 Gemin
51	*b*	5	50; 51; 52

Lustre of the stars in Orion.			
52		6	51 ; 52 , 60
53	κ	3	35 ; 53 29 ; 53 30 Hyd – 53 53 ; 34
54	χ^1	5	54 – – 57 54 – 62
55		6	49 – 55
56		6	56 ; 51
57	χ^2	5	54 – – 57 68 , 57
58	α	1	58 . 10 Canis min. 58 – – 87 Tauri 58 –, 10 Canis min.
59		6	60 –, 59
60		6	52 , 60 –, 59
61	μ	4	40 , 61 , 37
62	χ^3	6	54 – 62 – 64
63		6	66 . 63 66 ; 63
64	χ^4	6	62 – 64
65	χ^5	5.6	Does not exist.
66		6	66 . 63 66 ; 63
67	ν	4.5	67 ; 70 67 , 70
68		6	71 , 68 , 57
69	f^1	6	70 – 69 . 72
70	ξ	4.5	67 ; 70 –, 74 70 – 75 67 , 70 – 69
71		6	71 , 68
72	f^2	6	69 . 72
73	k^1	6	74 ; 73
74	k^2	6	70 –, 74 ; 73 75 , 74
75	l	6	70 – 75 , 74
76		6	Does not exist.
77		6	77 – 78
78		6	77 – 78

Notes to Andromeda.

1 By three observations of FLAMSTEED, page 130, 138, and 140, the polar-distance in the edition of 1725 requires + 9".

40 Is the same with 69 Piscium. FLAMSTEED observed it five times; twice among the stars of the constellation Pisces, and three times among those of Andromeda. See page 14, 134, 139, 149, and 210.

61 M. DE LA LANDE says is lost. See Mr. BODE's *Jahr-Buch* for 1794, page 97; but as the star is now in its place, it may perhaps be changeable, and ought to be looked after.

Notes to Bootes.

47 The RA in the British catalogue is only given to the nearest degree, and Mr. BODE and Mr. WOLLASTON, in their catalogues, have left it out; but FLAMSTEED has four complete observations of it, on page 166, 168, 414, and 415, and the star is called *k* in all of them.

Notes to Cancer.

26 Was not observed by FLAMSTEED. An observation on page 297 has occasioned the insertion of this star; but by correcting the time — 1', it will agree with two other observations of 22 Cancri on page 21 and 26. See Mr. BODE's *Jahr-Buch* for 1788, page 172.

56 This star has not been observed by FLAMSTEED, nor does it exist. Page 25 FLAMSTEED observed 55 Cancri with a memorandum, "*Hæc habet comitem sequentem ad austrum*;" which has probably occasioned the insertion of this star; but he had not then observed all the ρ's, and might possibly mean

to point out ϱ 53; which he afterwards observed on page 27. The stars are so near together that he might easily mistake *sequens* for *præcedens ad austrum.* FLAMSTEED in his observations calls 58 3d ϱ, 67 4th ϱ, and 70 5th ϱ; this shews that there is no authority for six ϱ's. See Mr. BODE's account of the same star in his *Jahr-Buch* for 1788, page 171.

71 " April 5, 1796. 71 Cancri is 15′ nearer to 78 and 15′ " farther from 68 than it is placed in Atlas."

73 and 74 Have not been observed by FLAMSTEED, nor do they exist. How they came to be inserted, does not appear to be satisfactorily accounted for by Mr. BODE in his *Jahr-Buch* for 1788, page 172. He gives us four observations of 62 and 63 Cancri; but FLAMSTEED has thirteen, and they are all perfect except the last on page 564.

Notes to Cepheus.

15 " October 25, 1796. 15 Cephei consists of two stars. " Both taken together for one, by the naked eye, give 14 . 15 " In the telescope they are 14 –, 15 – 15."

18 Has no time in FLAMSTEED's observations. " March 26, " 1797. 18 is a very little preceding 19. It is 1½ degree from " 17. The stars 18 , 20 and 19 are in a line which bends a " little at 18 towards the preceding side."

Notes to Corona Borealis.

21 In the British catalogue requires a correction of — 28′ 21″ in time of RA and — 14′ 55″ in PD. In the place where it is marked in Atlas, according to the erroneous catalogue, is no star; but very unaccountably it is also marked in its right place in the same Atlas. FLAMSTEED has four complete obser-

vations of it on page 167, 445, 477, and 478. Mr. WOLLASTON not being acquainted with the existence of 21 Coronæ in its right place, supposes zone 55°, that I have made a mistake in calling my double star VI. 18, very unequal; but in his corrections he gives us the place of a star, as he calls it "near ν," which is the real second ν of FLAMSTEED; who very particularly describes it on page 167, "*Duarum ad ν sequens et clarior*;" and this is the double star I have given in my catalogue as 21 Coronæ.

Notes to Navis.

1 There is no observation of this star; but in Miss HERSCHEL's manuscript catalogue, No. 92, is a star 2° more south, which has probably been calculated wrong, and has given occasion for its insertion; correcting, therefore, the PD of 1 Navis + 2°, the expression of its brightness is as I have given it.

17 There is no observation of this star; but if we correct the PD + 3°, it will then agree with No. 238 in Miss HERSCHEL's manuscript catalogue.

21 By FLAMSTEED's observation page 431, the PD of the British catalogue requires + 18′.

Notes to Orion.

12 FLAMSTEED never observed this star. It does not appear how it came to be inserted in the British catalogue.

26 FLAMSTEED never observed this star. An error of 20′ in PD in the calculation of one of the four observations of 25 Orionis, may have occasioned the insertion of it.

35 Is marked : : in the British catalogue; but FLAMSTEED

has seven complete observations of this star; therefore the marks : : should be out.

63 There is no observation of this star; but supposing an error of + 2′ 14″ of time in RA, and of + 0′ 22″ in PD, it will then agree with No. 33 of Miss HERSCHEL's manuscript catalogue. I have taken the comparative brightness of that star, supposing it to be 63.

64 and 65 Have no observation by FLAMSTEED; but their insertion has been accounted for by Mr. BODE in his *Jahr-Buch* for 1793, page 195. He mentions FLAMSTEED's two observations on page 17 and 94. There is a third on page 292, which confirms what Mr. BODE says. The 64 of which I give the brightness, is not far from the place assigned to it in the British catalogue. It is No. 1 in Miss HERSCHEL's manuscript catalogue.

76 There is no observation of this star. A mistake of 41′ in PD in calculating one of the four observations of 8 Monocerotis, might occasion its insertion.

WM. HERSCHEL.

Slough, near Windsor,
April 12, 1797.

XIV. *An Account of the Means employed to obtain an overflowing Well. In a Letter to the Right Honourable Sir* Joseph Banks, *Bart. K.B. P.R.S. from Mr.* Benjamin Vulliamy.

Read May 25, 1797.

SIR,

PERMIT me, in compliance with your request, to give you a short account of the well at Norland House, belonging to Mr. L. VULLIAMY; a work of great labour and expence, executed entirely under my direction, and finished in November, 1794.

Before I began the work, I considered that it would be of infinite advantage, should a spring be found strong enough to rise over the surface of the well; and though I thought it very improbable, yet I resolved to take from the beginning the same precautions in doing the work, as if I had been assured that such a spring would be found. But although this very laborious undertaking has succeeded beyond my expectation, yet from the knowledge I have acquired in the progress of the work, I am of opinion that it will very seldom happen that the water will rise so high; nor will people, I believe, in general, be so indefatigable as I have been in overcoming the various difficulties that did and ever will occur, in bringing such a work to perfection.

In beginning to sink this well, which has a diameter of four feet, the land springs were stopped out in the usual manner, and the well was sunk and steined to the bottom. When the

workmen had got to the depth of 236 feet, the water was judged not to be very far off, and it was not thought safe to sink any deeper. A double thickness of steining was made about 6 feet from the bottom upwards, and a borer of $5\frac{1}{4}$ inches diameter was made use of. A copper pipe of the same diameter with the borer was driven down the bore-hole to the depth of 24 feet, at which depth the borer pierced through the rock into the water; and by the manner of its going through, it must probably have broken into a stratum containing water and sand. At the time the borer burst through, the top of the copper pipe was about 3 feet above the bottom of the well: a mixture of sand and water instantly rushed in through the aperture of the pipe. This happened about two o'clock in the afternoon, and by twenty minutes past three o'clock the water of the well stood within 17 feet of the surface. The water rose the first 124 feet in eleven minutes, and the remaining 119 feet in one hour and nine minutes. The next day several buckets of water were drawn out, so as to lower the water 4 or 5 feet; and in a short time the water again rose within 17 feet of the surface. A sound-line was then let down into the well in order to try its depth. To our great surprise the well was not found by 96 feet so deep as it had been measured before the water was in it; and the lead brought up a sufficient quantity of sand to explain the reason of this difference, by shewing that the water had brought along with it 96 feet of sand into the well. Whether the copper pipe remained full of sand or not, is not easy to be determined; but I should rather be inclined to think it did not.

After the well had continued in the same state several days, the water was drawn out so as to lower it 8 or 10 feet; and

it did not rise again by about a foot so high as it had risen before. At some days interval water was again drawn out, so as to lower the water as before; which at each time of drawing rose less and less, until after some considerable time it would rise no more; and the water being then all drawn out, the sand remained perfectly dry and hard. I now began to think the water lost; and, consequently, that all the labour and expence of sinking this well, which by this time were pretty considerable, had been in vain. There remained no alternative but to endeavour to recover it by getting out the sand, or all that had been done would be useless; and although it became a more difficult task than sinking a new well might have been, yet I determined to undertake it, because I knew another well might also be liable to be filled with sand in the same manner that this was. The operation of digging was again necessarily resorted to, and the sand was drawn up in buckets until about 60 feet of it were drawn out, and, consequently, there remained only 36 feet of sand in the well: that being too light to keep the water down, in an instant it forced again into the well with the same violence it had done before; and the man who was at the bottom getting out the sand, was drawn up almost suffocated, having been covered all over by a mixture of sand and water. In a short time the water rose again within 17 feet of the surface, and then ceased to rise, as before. When the water had ceased rising, the sounding-line was again let down, and the well was found to contain full as much sand as it did the first time of the water's coming into it.

Any further attempt towards recovering the water appeared now in vain; and most people would, I believe, have abandoned the undertaking. I again considered that the labour and the

expence would be all lost by so doing; and I determined without delay to set about drawing the sand out through the water, by means of an iron box made for that purpose, without giving it time to harden as before. The labour attending on this operation was very great, as it was necessary continually to draw out the water, for the purpose of keeping it constantly rising through the sand, and thereby to prevent the sand from hardening. What rendered this operation the more discouraging was, that frequently after having drawn out 6 or 7 feet of sand in the course of the day, upon sounding the next morning the sand was found lowered only 1 foot in the well, so that more sand must have come in again. This, however, did not prevent me from proceeding in the same manner during several days, though with little or no appearance of any advantage arising from the great exertions we were making. After persevering, however, for some considerable time, we perceived that the water rose a little nearer to the surface, and I began to entertain some hopes that it might perhaps rise high enough to come above the level of the ground; but when the water had risen a few feet higher in the well, some difficulties occurred, occasioned by accidental circumstances, which very much delayed the progress of the work; and it remained for a considerable time very uncertain whether the water would run over the top of the well or not.

These difficulties being at length surmounted, we continued during several days the process before mentioned, of drawing out the sand and water alternately; and I had the satisfaction of seeing the water rise higher and higher, until at last it ran over the top of the well, into a temporary channel that conveyed it into the road. I then flattered myself that every difficulty

was overcome; but a few days afterwards I discovered that the upper part of the well had not been properly constructed, and it became necessary to take down about 10 feet of brick-work. The water, which was now a continued stream, rendered this extremely difficult to execute. I began by constructing a wooden cylinder 12 feet long, which was let down into the well, and suspended to a strong wooden stage above, upon which I had fixed two very large pumps, of sufficient power to take off all the water that the spring could furnish, at 11 feet below the surface. The stage and cylinder were so contrived as to prevent the possibility of any thing falling into the well; and I contrived a gage, by which the men upon the stage could always ascertain to the greatest exactness the height of the water within the cylinder. This precaution was essentially necessary, in order to keep the water a foot below the work which was doing on the outside of the cylinder, to prevent the new work from being wetted too soon. After every thing was prepared, we were employed eight days in taking down 10 feet of the wall of the well, remedying the defects, and building it up again; during which time ten men were employed, five relieving the other five, and the two pumps were kept constantly at work during one hundred and ninety-two hours. By the assistance of the gage, the water was never suffered to rise upon the new work until it was made fit to receive it. When the cylinder was taken out, the water again ran over into the temporary channel that conveyed it into the road.

The top of the well was afterwards raised 18 inches, and constructed in such a manner as to be able to convey the water

five different ways at pleasure, with the power of being able to set any of these pipes dry at will, in order to repair them whenever occasion should require. The water being now entirely at command, I again resolved upon taking out more sand, in order to try what additional quantity of water could be obtained thereby. I cannot exactly ascertain the quantity of sand taken out, but the increase of water obtained was very great; as instead of the well discharging thirty gallons in a minute, the water was now increased to forty-six gallons in the same time.

If you think, Sir, that the above account of an overflowing well, the joint production of nature and art, is deserving your attention, I feel myself much gratified in the pleasure I have in giving you this description of it; and have the honour of being, with the greatest regard, SIR, &c.

B. VULLIAMY.

EXPLANATION OF THE PLATE. (Tab. VII.)

Fig. 1.

a Top of the well, with the water running over.
b b Ground line.
c Sand lying in the well.
d Copper pipe.
f f f f f f Steining of the well.
g g Double steining six feet from the bottom upwards.
b Stratum which the end of the copper pipe was driven into.

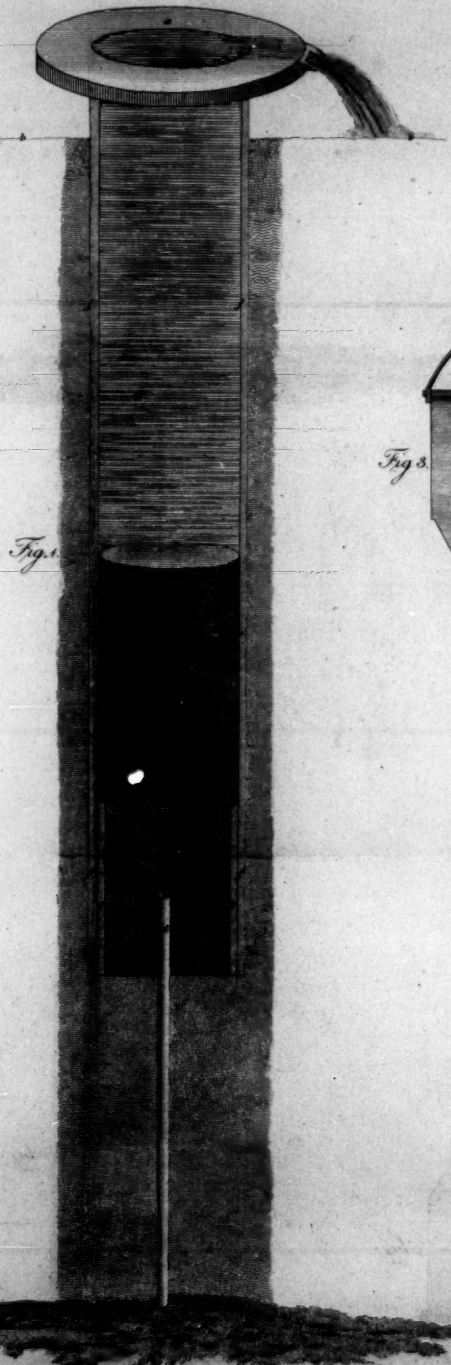


Fig. 1.

Fig. 3.

Fig. 2.

Fig. 2. and 3.

Iron box for drawing sand out of the well, weighing about 60lbs. one foot square, and two feet nine inches long.

a Handle of the box.

b A flap or door, which opens inwards by a joint at *c*. There is another door like this on the other side.

c The joint.

d The centre or pin of the joint.

XV. *Observations of the changeable Brightness of the Satellites of Jupiter, and of the Variation in their apparent Magnitudes; with a Determination of the Time of their rotatory Motions on their Axes. To which is added, a Measure of the Diameter of the Second Satellite, and an Estimate of the comparative Size of all the Four.* By William Herschel, *LL.D. F.R.S.*

Read June 1, 1797.

It may be easily supposed when I made observations on the brightness of the 5th satellite of Saturn, by way of determining its rotation upon its axis, and found that these observations proved successful, that I should also turn my thoughts to the rest of the satellites, not only of Saturn, but likewise of Jupiter, and of the Georgian planet. Accordingly I have from time to time, when other pursuits would permit, attended to every circumstance that could forward the discovery of the rotation of the secondary planets; especially as there did not seem to lie much difficulty in the way. For since I have determined, by observation, that the 5th satellite of Saturn is in its rotation subject to the same law that our moon obeys, it seems to be natural to conclude that all the secondary planets, or satellites, may probably stand in the same predicament with the two I have mentioned; consequently a few observations that coincide with this proposed theory, will go a good way towards a confirmation of it.

I had another point in view when I made the observations

which are contained in this paper. It was an attempt to avail myself of the abundant light and high powers of my various telescopes, to examine the nature and construction of the bodies of the satellites themselves, and of their real magnitudes. Here phænomena occurred that will perhaps be thought to be remarkable, and even inconsistent or contradictory. So far from attempting to lessen the force of such animadversions, I shall be the first to point out difficulties, in order that future observations may be made to resolve them.

Perhaps it would have been better to delay the communication of these observations, till I had continued them long enough to be able to account for things which at present must be left doubtful. But as in final conclusions to be drawn from astronomical observations, we ought to take care not to be precipitate; so on the other hand I am perhaps too scrupulous in satisfying myself, and should probably require the observations of several years before I could venture to be decisive. It will also be seen by the dates of the first observations, that a further delay in the communication cannot be adviseable; since much information may possibly be gained by throwing open, to other observers, the road it will be eligible to take for a satisfactory investigation of the subject; especially as we have reason to congratulate ourselves on the spirit of observation, and increase of large instruments, that seem to have taken place in various parts of Europe.

I shall now transcribe the observations from my journals. They are as follows.

OBSERVATIONS.

A remarkable Conjunction of two Satellites of Jupiter.

May 14, 1790. $11^h\ 30'\ 10''$; correct sidereal time. The 2d and 3d satellites of Jupiter are so closely in conjunction, that with a 7-feet reflector, charged with a magnifying power of 350, I cannot see a division between them.

$11^h\ 34'\ 10''$. The shadow of the 1st satellite is still upon the disc of the planet.

Intenseness of Light and Colour of the Satellites.

July 19, 1794. $17^h\ 12'\ 47''$. 7-feet reflector. The 1st satellite of Jupiter is of a very intense bright, white, shining light. It is brighter than the 2d or 4th. I speak only of the light, and not of the size.

The colour of the 4th satellite is inclining to red. In brightness it is very nearly, but not quite equal to the 2d. I make no allowance for its being farther from the bright disc of Jupiter than the 2d.

10-feet reflector, power 170. The 3d satellite is just gone upon the body; before it went on, it appeared to me to be smaller than usual.

The 2d satellite is of a dull, ash-colour; not in the extreme, but rather inclining to that tint.

July 21, 1794. $16^h\ 56'\ 45''$. 10-feet reflector; power 170. The 3d satellite of Jupiter is round, large, and well defined. It is very bright, and its light is very white.

The 4th satellite is also round, large, and well defined. I estimate its magnitude in proportion to that of the 3d satellite to be as 4 to 5. Its light is not white, but inclined to orange.

Brightness and Diameter distinguished.

July 26, 1794. $17^h\ 14'\ 41''$. 10-feet reflector; power 170. The 4th satellite is very dim. It is of a pale, dusky, reddish colour.

The 2d satellite is of a bright, white colour.

The 3d satellite is very bright, and white.

The 1st satellite is very brilliant, and white.

$17^h\ 22'\ 41''$. *The Magnitudes with* 240.

The 3d satellite is the largest.

The 2d satellite is the smallest.

With 300.

The 4th satellite is a very little larger than the 2d, though less bright.

The 1st satellite is larger than either the 4th or 2d.

With 400, the order of the magnitudes is 3 1 4 2.

With the same power, the order of the light is 3 1 2 4.

Now and then it appeared to me doubtful whether the 4th satellite was larger than the 2d; and as their light is of an unequal intensity, it is difficult without much attention, to be decisive about the magnitudes.

Diameter of the second Satellite by entering on the Disc of the Planet.

July 28, 1794. $17^h\ 25'\ 40''$. 10-feet reflector; power 170. The 2d satellite is nearly in contact with the following limb of Jupiter.

$17^h\ 29'\ 40''$. It seems to be very near the contact. With 300, very near the contact.

$17^h\ 30'\ 40''$. It seems to be in contact. It is brighter than that part of Jupiter where it enters.

$17^h\ 31'\ 40''$. It is more than half entered.

$17^h\ 33'\ 40''$. It seems to be nearly quite entered. Its superior brightness makes it seem protuberant.

$17^h\ 34'\ 40''$. It is certainly quite entered.

$17^h\ 35'\ 25''$. I see a little of the disc of Jupiter on the outside of the satellite, equal to about $\frac{1}{4}$ of its diameter.

$17^h\ 39'\ 40''$. The 3d satellite is very bright, and of its usual colour.

The 4th satellite is faint, and also of its usual colour.

The 1st satellite is very bright, and the light of it is of its usual intenseness.

The Magnitudes with 300.

The diameter of the 4th seems to be to that of the 3d, as 2 to 3; or perhaps more exactly, as 3 to 5.

The diameter of the 4th satellite exceeds that of the 1st a very little.

With 400.

With this power the diameter of the 4th satellite certainly exceeds that of the 1st.

The diameter of the 4th, is to that of the 3d, as 3 to 5.

July 30, 1794. $19^h\ 1'\ 37''$. 10-feet reflector; power 300. The 4th satellite of Jupiter is a little larger than the 1st. It is of its usual colour.

The 2d is less than the 1st.

The 3d is larger than the 4th.

July 31, 1794. $17^h\ 18'\ 38''$. 10-feet reflector; power 170. The four satellites of Jupiter are very favourably placed for my purpose.

The 1st is less bright than the 2d; it is a very little larger than the 2d: the difference in the size is but barely visible.

The light of the 2d is very intense and white.

The light of the 3d is very intense and bright.

The light of the 4th is dull; and seems to be inferior to the usual proportion it bears to the other satellites.

$18^h\ 38'\ 38''$. *With* 300.

The 4th satellite is larger than the 1st.

The 2d satellite is a little larger than the 1st, or at least equal to it.

The 3d is undoubtedly the largest. The order of the magnitudes therefore is, 3 4 2 1.

My Brother, Alexander Herschel, looked at the satellites, and estimated the order of their magnitudes exactly the same; though he was not present when I made the foregoing estimations.

August 1, 1794. $17^h\ 38'\ 37''$. 10-feet reflector; power 170. The light of all the four satellites is very brilliant, the evening being very fine.

With 300.

The northmost and farthest of the two satellites which are in conjunction, is the smallest: I suppose it to be the 2d.

The southmost and nearest of the two satellites in conjunction, is the next in size: I suppose it to be the 1st.

The 4th satellite is a little larger than the largest of the two satellites which are in conjunction; but the difference is only visible with a great deal of attention.

The 3d satellite is much larger than the 4th.

August 9, 1794. $17^h\ 56'\ 32''$. 10-feet reflector; power 170. The light of the 1st satellite is very intense and white.

The light of the 2d satellite is also pretty intense and white.

The light of the 3d satellite is neither so intense nor so white as that of the 1st.

The light of the 4th is dull and of a ruddy tinge.

With 300, and 400, the second is the least, and the 3d is the largest. I am in doubt whether the 4th or the 1st is largest; with 600, I suppose the 1st to be larger than the 4th.

September 30, 1795. $20^h\ 15'\ 17''$. 7-feet reflector; power 210. Order of the magnitudes of the satellites of Jupiter 3 - 2 , 1 , 4. Power 110. 3 - 2 , 1 . 4. With 460, 3 - 2 , 1 , 4.*

October 2, 1795. $20^h\ 18'\ 22''$. 7-feet reflector; power 287. Jupiter's satellites 3 - - 2 - 1 , 4. The 2d and 3d satellites are not yet in conjunction.

$20^h\ 43'\ 22''$. The conjunction between the 3d and 2d satellites is past. The distance between them is now one diameter of the 3d.

August 18, 1796. $18^h\ 47'\ 21''$. 7-feet reflector; power 287. The 4th satellite is less bright than the 1st; notwithstanding

* Here, in order to denote the different magnitudes of the satellites, I used the notation which has been explained in my *First Catalogue of the comparative Brightness of the Stars*. See Phil. Trans. for the year 1796, Part I. page 189.

the latter is so near the planet as to have its light overpowered by Jupiter, while the 4th is at a great distance. I mean light or brightness, not magnitude.

The 1st is very bright.

September 15, 1796. $19^h\ 25'\ 25''$. 10-feet reflector; power 300. The 2d satellite of Jupiter is a little less than the 1st.

The 3d is much larger than any of the rest.

Power 600. The difference in the magnitude of the 1st and 2d satellites, with this power, is pretty considerable.

September 21, 1796. $19^h\ 24'\ 5''$. 10-feet reflector; power 600. The shadow of the 1st satellite is upon one of the dark belts of Jupiter.

In order to use very high powers with this telescope, I tried it upon the double star ζ Aquarii with 1200. The air is very tremulous, but I see now and then the two stars of this double star very well defined.

With the same power, the satellites of Jupiter are very large, but not so well defined as the above star.

The Brightness of the Satellites compared to the Belts and Disc of the Planet.

The 1st satellite, which is lately come off the southern belt, is nearly of the same brightness with that belt; power 600. With 400, it is nearly as bright as the brighter part of the planet, or rather a mean between the belt and the planet.

The 2d satellite is considerably bright; its colour is whiter than that of the 1st; it is however not so white as the colour of the bright part of Jupiter.

The colour of the 4th satellite is as dingy as that of the belt; very much less bright and less white than that of the 2d.

The brightness of the 3d satellite is not intense; its colour, however, is white, though not so white as the bright part of the planet.

September 24, 1796. $20^h\ 55'\ 24''$. 10-feet reflector; power 600. The 1st satellite of Jupiter is very bright, and of a white colour; it is also very large.

The 2d satellite is faint and bluish; its light is not much brighter than that of the belt.

The 3d satellite is pretty bright; its light is whitish. It seems to be comparatively less than it ought to be; or rather, its apparent smallness is owing to the uncommon largeness of the 1st.

The 1st satellite, with 200, compared to the 3d, is proportionally larger than I have seen it before.

September 30, 1796. $20^h\ 8'\ 4''$. 10-feet reflector; power 600. The satellites of Jupiter are well defined, and the night is beautiful.

The 3d satellite, in proportion to the 1st, is much larger than it was September 24. I ascribe the change to an apparent diminution of the 1st.

$20^h\ 30'\ 4''$. The 1st satellite is evidently less in proportion to the 3d, than it was September 24.

The 2d satellite is considerably bright; its light is whitish; much brighter than the belt, but not so bright as the bright part of the disc. Its magnitude is less than that of the 4th; but its light is considerably superior.

The 3d satellite is remarkably well defined. Its light is considerably brighter than that of the belts.

The magnitude of the 1st satellite exceeds that of the 2d. It is nearly equal to that of the 4th.

$22^h\ 58'\ 4''$. Appearances as before.

October 15, 1796. $21^h\ 23'\ 42''$. 10-feet reflector; power 600. The 2d satellite is uncommonly bright; its apparent magnitude is also larger than usual.

The 4th satellite is very faint; it is not brighter than the belt, but is of a bluish, ruddy colour.

The apparent magnitude of the 2d satellite, after long looking, is very nearly equal to that of the 1st; but at first sight it seems to be larger, owing to its superior brightness.

The apparent diameter of the 2d satellite is certainly larger than that of the 4th.

$23^h\ 55'\ 42''$. The light of the 1st satellite, compared to that of the 2d, is considerably increased since the last observation. It is now nearly as bright as the 2d.

October 16, 1796. $0^h\ 23'\ 49''$. 10-feet reflector; power 600. The 1st, 2d, and 3d satellites of Jupiter seem all considerably bright.

The 3d is much larger than the 1st, and the 1st a little larger than the 2d.

The intensity of the light seems to be pretty equal in all the three; that of the 2d, however, is perhaps a little stronger than that of the 1st; for, notwithstanding its apparent less diameter, it seems to make as strong an impression as the 1st.

October 25, 1796. $21^h\ 44'\ 48''$. 10-feet reflector; power 600. The 1st satellite of Jupiter, compared to the 3d, is small.

The 3d satellite is bright and large.

The 2d is brighter than the 1st. Compared to its usual brightness and magnitude, it is very bright and small.

The 1st satellite, compared to its usual brightness and magnitude, is faint and small.

The air is so tremulous that the power of 600 is too high, and the necessary uniformity required in these observations will not permit a lower to be used. Perhaps one of 400 might be more generally employed; and it may be proper to use it constantly.

November 3, 1796. $23^h\ 55'\ 47''$. 10-feet reflector; power 600. The 4th satellite of Jupiter is large and bright.

The 3d satellite is large and bright.

The 1st satellite is pretty small, and not very bright.

The 2d satellite is small, and considerably bright.

The brightness and magnitude of each satellite refer to its own usual brightness and magnitude.

Before we can proceed to draw any conclusions from these observations, we ought to take notice of many causes of deception, and of various difficulties that attend the investigation of the brightness of the satellites.

The difference in the state of the atmosphere between two nights of observation, cannot influence much our estimation of the brightness of a satellite, provided we adopt the method of comparative estimations. If we endeavour by much practice to fix in our mind a general ideal standard of the brightness of each satellite, we shall find the state of the atmosphere in different nights very much disposed to deceive us; but if we learn to acquire a readiness of judging of the comparative brightness of each satellite with respect to the other three, we may arrive at much more precision, since the different disposition of the air will nearly affect all the satellites alike. But here, as we get rid of one cause of deception, we fall under the penalty of another. The situation of those very satellites to which we are

to refer the light of the satellite under estimation, being changeable, permits us no longer to trust to their standard, without a full scrutiny of the causes that may have produced an alteration in them.

In the foregoing observations it will also be seen, that I attempted to compare the intenseness of the light of the satellites with the different brightness of the disc of Jupiter; but these endeavours will always fail, on account of the little assurance we can have that the parts of the disc, setting aside its quick rotation, will remain for any time of the same lustre.

A very material difficulty arises from the magnifying power we use in our estimations. If it be a low one, such as for instance 180 (for a lower should not even be attempted), then we run the risk of being disappointed in bright nights by the sparkling of the brilliant light of the satellites. Besides, we cannot then see the bodies of them, and judge of their comparative magnitude, with the same power that we view their light. If we choose a high magnifier, we shall be often disappointed in the state of the atmosphere, which will of course occasion an interruption in the series of our observation, of which the regular continuance is of the greatest consequence. If we change our power according to the state of the atmosphere, we introduce a far worse cause of confusion; for it will be next to impossible to acquire, for each magnifying power, an ideal standard of comparative brightness to which we can trust with confidence.

If the magnitudes are not attended to, and carefully contradistinguished from the intenseness of light, we shall run into considerable error, by saying that a satellite is large, when we mean to express that it is bright. It is so common to call stars that are less bright than others, small, that we must be careful

to avoid such ambiguities, when the condition of the satellites is under investigation. Nor is it possible to throw the size and light into one general idea, and take the first *coup d'oeil* in looking at them, to decide about the general impression this compound may make. When our attention is forcibly drawn by a considerable power to the apparent size of the satellite we are looking at, its brightness can no longer be taken in that general way, but must be abstracted from size.

Let us now see what use may be drawn from the observations I have given.

It appears in the first place very obviously, that considerable changes take place in the brightness of the satellites. This is no more than might be expected. A variegated globe, whether terraqueous like the earth, or containing regions of soil of an unequal tint, like that side of the moon which is under our inspection, cannot, in its rotation, present us with always the same quantity of light reflected from its surface.

In the next place, the same observations point out what we could hardly expect to have met with; namely, a considerable change in the apparent magnitude of the satellites. Each of them having been at different times the standard to which another was referred, we cannot refuse to admit a change so well established, singular as it may appear.

The first of these inferences proves that the satellites have a rotatory motion upon their axes, of the same duration with their periodical revolutions about the primary planet.

The second either shews that the bodies of the satellites are not spherical, but of such forms as they have assumed by their quick periodical and slow contemporary, rotatory motions, and which forms in future may become a subject for mathematical

investigation; or it may denote, in case geometrical researches should not countenance a sufficient deviation from the spherical form, that some part of the discs of these satellites reflects hardly any light, and therefore in certain situations of the satellite makes it appear of a smaller magnitude than in others.

Here then we see evidently that a considerable field for speculation, as well as observation, is opened to our view; and almost every attempt to enter upon the work must seem premature, for want of more extended observations. However, from those that have been given, such as they are, I will shew how far we may be authorized to say, that the satellites revolve on their axes in the same time that they perform a periodical revolution about the planet.

I shall take the usual method of throwing the observations of each satellite into a graduated circle. The zero of the degrees into which I suppose it divided, is in all observations assumed to be in the place of the geocentric opposition.

In order to bring these observations to the circle, the places of the satellites have been calculated from my own tables of the mean motion in degrees, and according to epochs continually assumed from the geocentric conjunctions pointed out in the configurations of the Nautical Almanac; and the nearest of these conjunctions have been always used. This method is fully sufficient for the purpose, as greater precision in the calculation is not required.

The observations extend from July 19, 1794, to November 3, 1796; and therefore include a period which takes in 470 rotations of the 1st satellite; 234 of the 2d; 116 of the 3d; and 50 of the 4th: that is, provided we admit that these rotations

are performed in the same time the satellites revolve in their orbits.

In the following table are the calculated places of the satellites; the correct sidereal times, given with the observations, having been turned into mean time.

Table of the Positions of the four Satellites of Jupiter at the Time of the Observations.

Time of Observ.	I	II	III	IV
1794.	°	°	°	°
July 19d 9h 21′	127	348	179	46
July 21. 8. 57			278	89
July 26. 8. 56	124	333	169	205
July 28. 8. 59	171	176	270	248
July 30. 10. 27	231	25	13	292
July 31. 8. 40	59	118	60	312
Aug. 1. 8. 56	265	221	111	334
Aug. 9 8. 42	83	310	152	138
1795.				
Sept. 30. 7. 37	294	62	219	100
Oct. 2. 7. 32	341	264	319	143

Time of Observ.	I	II	III	IV
1796.	°	°	°	°
Aug. 18d 8h 21′	115			191
Sept. 15. 7. 44	56	328	198	
Sept. 21. 7. 19	172	214	138	210
Sept. 24. 8. 38	74	163	305	275
Sept. 30. 7. 27	206	46	244	36
Oct. 15. 7. 44	28	130		5
Oct. 15. 10. 15	49			
Oct. 16. 10. 39	256	243	334	
Oct. 25. 7. 25	201	72	59	
Nov. 3. 9. 0	306	270	151	58

It will be necessary now to explain in what manner, with the assistance of this table, the observations of the brightness and magnitudes of the satellites have been reduced to the expressions they bear in the four circles of the figures contained in Tab. VIII. and IX. By way of uniformity I judged it would be best to reduce the estimations of magnitude to those of brightness; as it may be justly supposed that when a satellite is at any given time larger in proportion to another than it was at another time, it will also be brighter than it was at that other time, due regard being had to the light of the satellite to which its magni-

tude has been compared. To manage the space allotted to the figure advantageously, I have used the abbreviations formerly employed in my catalogue of Nebulæ, *v* B, *c* B, B, *p* B, *p* F, F, *c* F, *v* F, for all the gradations of light that are necessary to express the brightness of the satellites at the time of observation. It will be easily remembered that B and F mean bright and faint; and *p*, *c*, *v*, stand for pretty, considerably, and very.

Now, when the observation mentions the brightness of the satellite, I place it in the figure as it is given. In that of the first, for instance, July 19, 1794, we find the satellite called very bright; I therefore put down in fig. 1. (Tab. VIII.) at 127 degrees, *v* B. But where the brightness is not expressed, I have recourse to the comparative magnitude, if that can be had. By fig. 3. (Tab. IX.) it appears that the 2d satellite is less subject to a change of brightness than either the 1st or 4th: it becomes, for that reason, a pretty good standard for the light of these other satellites. Therefore, in the observation of October 2, 1795, for instance, where the 1st satellite is described as undoubtedly less than the 2d, I put down very faint, or *v* F, at 341 degrees of the circle in fig. 1.; for in the observation of July 19, before mentioned, when the satellite was called very bright, it was at the same time described as undoubtedly larger than the 2d. In this case, as regard must be had to the relative state of the satellite we refer to, the four figures I have given will assist us in determining the condition of the light of the satellite we wish to admit as a standard.

In reducing the 2d satellite to the circle, I have generally used a reference to the magnitude of the 1st, where marks of

brightness were wanting; and sometimes also to the magnitude of the 4th, and even of the 3d.

The 3d satellite can hardly be ever compared to any but the 2d in magnitude; and this only in its degree of excess.

The magnitude of the 4th satellite has been generally compared with that of the 1st; and also sometimes with that of the 2d.

To make an application of the contents of the figures, will now require little more than a bare inspection of them.

The 1st satellite appears evidently to have a rotation upon its axis that agrees with its revolution in its orbit. It cannot be supposed that, in the course of 470 revolutions, all the bright observations could have ranged themselves in one half of the orbit, while the faint ones were withdrawn to the other. The satellite appears in the middle of the duration of its brightness, when it is nearly half way between its greatest eastern elongation, in the nearest part of its orbit; or when advancing towards its conjunction. I have pointed out this circumstance by a division with dotted lines, and the words bright and faint, inserted within the circle, fig. 1. This satellite, therefore, revolves on its axis in $1^d\ 18^h\ 26'.6$.

The 2d satellite, though much less subject to change, on account, as we may suppose, of having only a small region on its body which reflects less light than the rest; has, nevertheless, its rotation directed by the same law with the 1st. It will hardly be necessary to take notice of a single deviation which occurs at 163 degrees, fig. 2.; as from the proximity of the satellite to the conjunction, a mistake in the estimation may easily take place. I generally made it a rule not to make

allowance for the influence of the superior light of the planet; but it seems that we can hardly abstract sufficiently on such occasions. Two similar cases occur, in fig. 3. at 179; and fig. 4. at 5 degrees.

It is indeed not impossible but that occasional changes, on the bodies of the satellites themselves, may occasion some temporary irregularity of their apparent brightness: it will, however, not be necessary to make such an hypothesis, till we have better authority for it. The brightest side of this satellite is turned towards us when it is between the greatest eastern elongation and the conjunction. It revolves, consequently, on its axis in $3^d\ 18^h\ 17',9$.

The 3d satellite suffers but little diminution of its brightness, and is in full lustre at the time of both its elongations. It is however not impossible but that, after having recovered its light, on the return from the opposition, it may suffer a second defalcation of it in the nearest quadrant about half way towards the conjunction. The two independent observations at 151 and 152 degrees, fig. 3. seem to give some support to this surmise. It revolves on its axis in $7^d\ 3^h\ 59',6$.

The 4th satellite presents us with a few bright views when it is going to its opposition, and on its return towards the greatest eastern elongation; but otherwise it is generally overcast. Its colour also is considerably different from that of the other three; and it revolves on its axis in $16^d\ 18^h\ 5',1$.

It will not be amiss to gather into one view, all the observations that relate to the colour of the satellites.

The 1st is white; but sometimes more intensely so than at others.

The 2d is white, bluish, and ash-coloured.

The 3d is always white; but the colour is of different intensity, in different situations.

The 4th is dusky, dingy, inclining to orange, reddish and ruddy at different times; and these tints may induce us to surmise that this satellite has a considerable atmosphere.

I shall conclude this paper with a result of the observation of the diameter of the second satellite, taken by its entrance upon the disc of the planet, July 28, 1794, and marked in fig. 2. at 176 degrees.

The duration by the observation is fixed at 4 minutes; in which time it passes over an arch in its orbit of 16′ 52″,9. Now as its distance from the planet is to its distance from the earth, so is 16′ 52″,9 to the diameter of the satellite; or the mean distance of the 2d satellite may be rated, with M. DE LA LANDE, at 2′ 57″, or 177″. Then putting this equal to radius, we shall have the following analogy. Radius is to 177″, as the tangent of 16′ 52″,9 is to the angle, in seconds, which the diameter of the second satellite subtends when seen from the earth. And by calculation, this comes out 0″,87; that is less than nine-tenths of a second.

I have not been scrupulously accurate in this calculation, as the real distance of Jupiter at the time of observation should have been computed, whereas I have contented myself with the mean distance. Nor am I very confident that the angle of the greatest elongation, admitted to be 2′ 57″, is quite accurate; but I judged it unnecessary to be more particular, because the time of my observation in the beginning of the transit upon the disc, I find was only taken down in whole minutes of the clock. The end, however, is more accurately determined, by the observation which was made 45″ after the immersion; when a

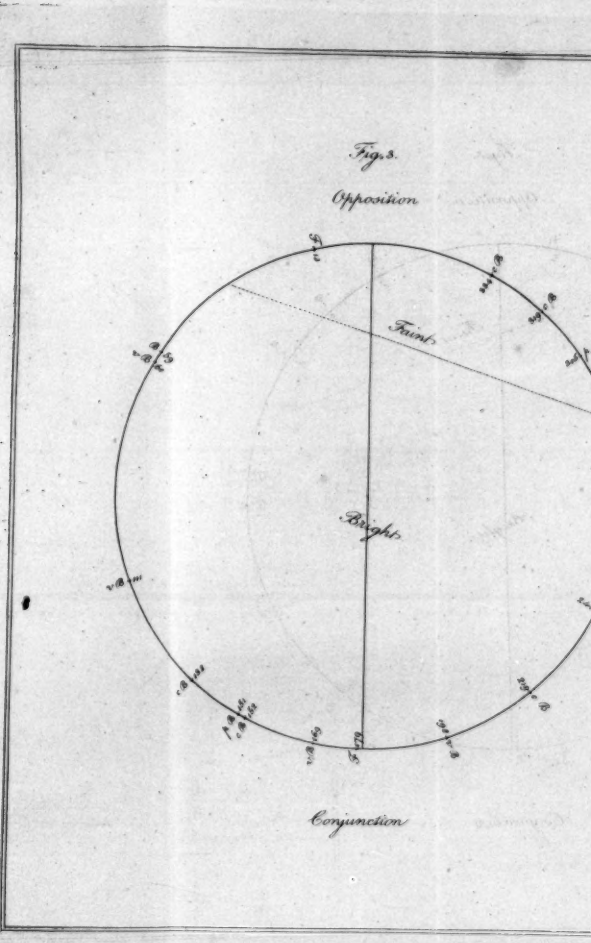

Figs.
Opposition
Faint
Bright
Conjunction

Fig. 4.

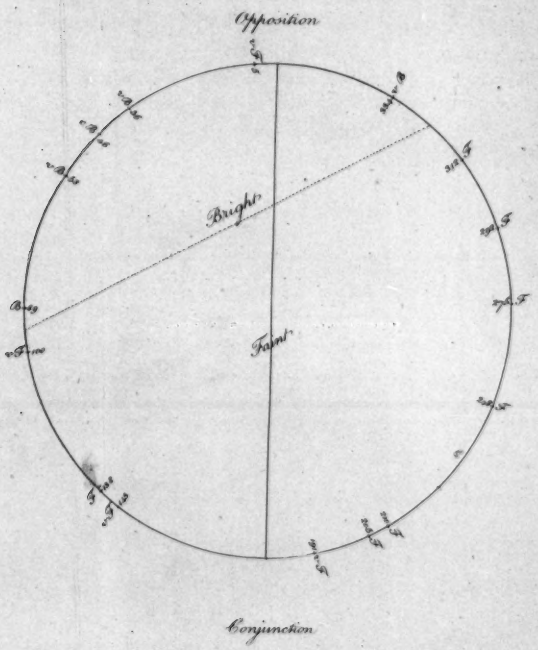


Fig. 1.

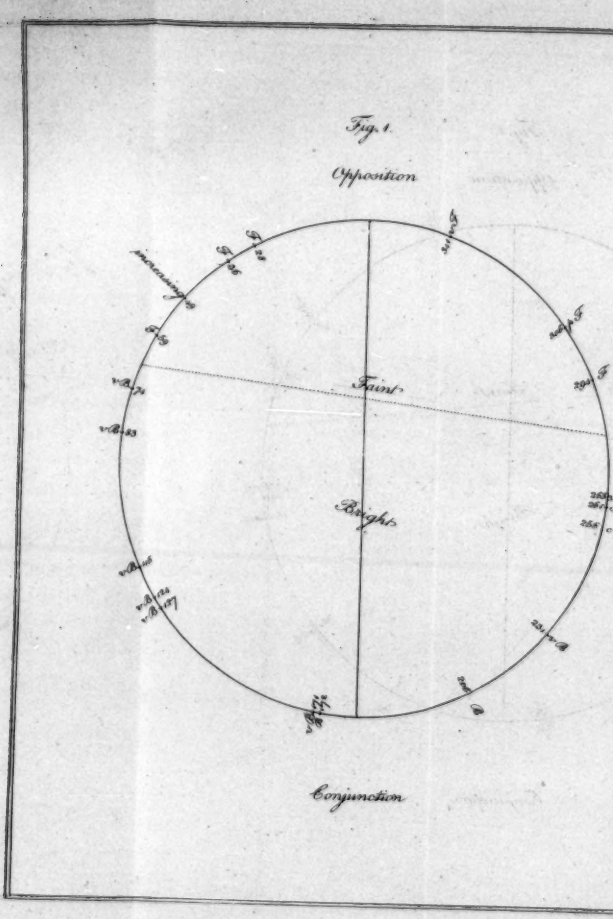
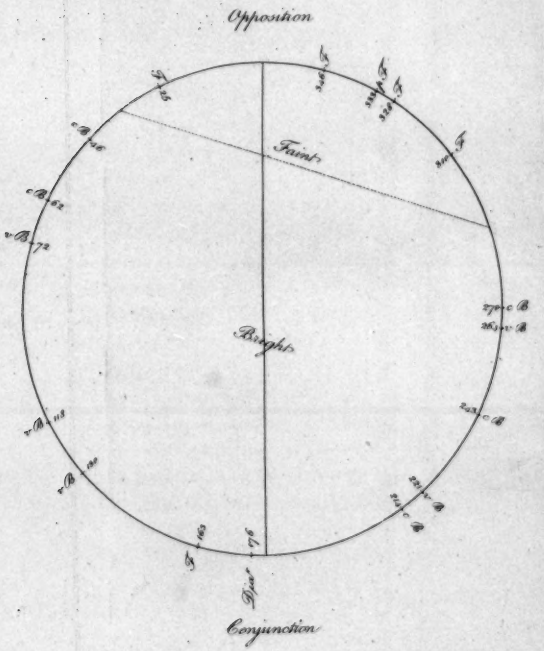
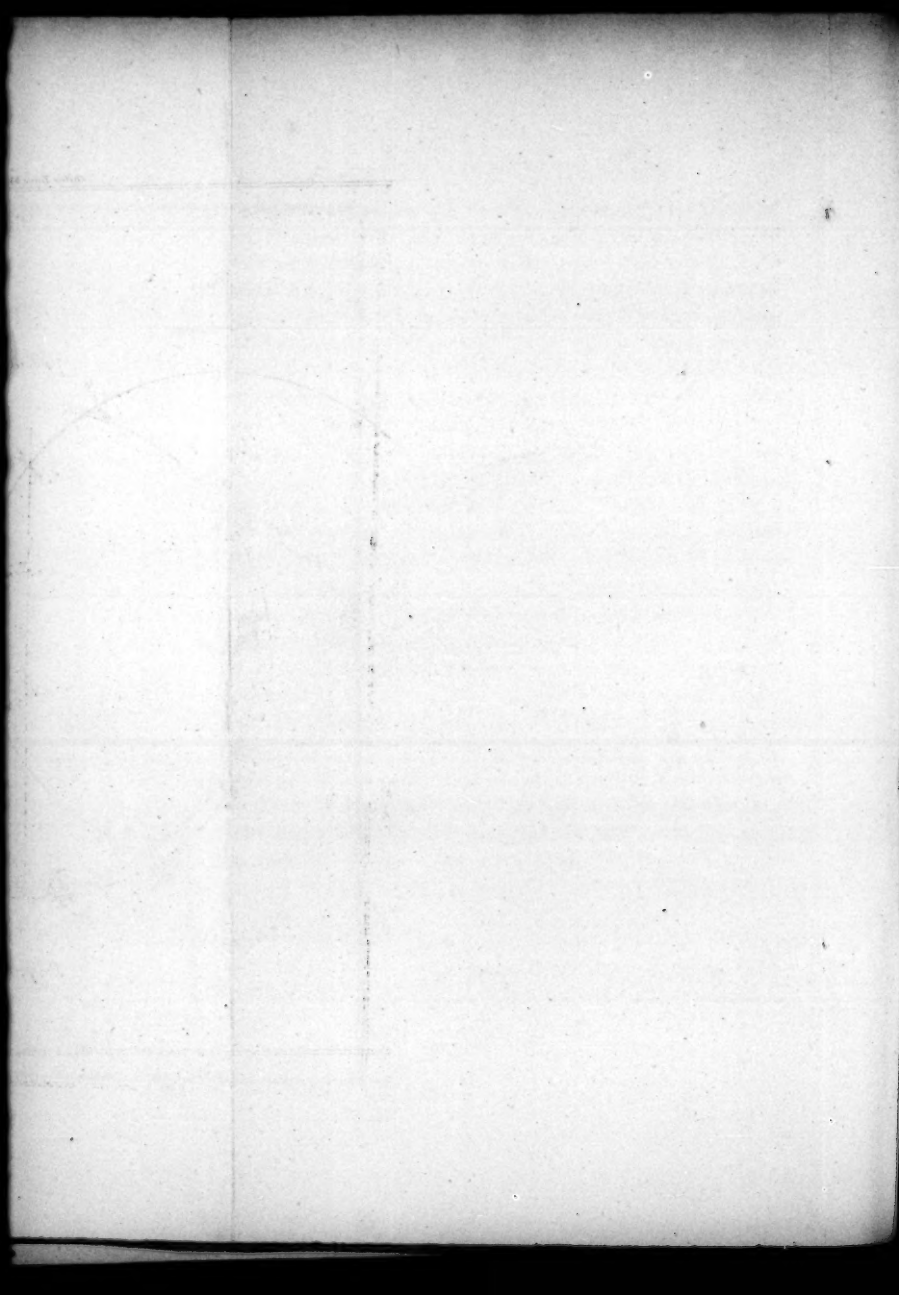


Fig. 2.





part of the disc, equal to about $\frac{1}{4}$ of the diameter of the satellite, is said to be visible. It seems that observations of this kind, made with very good telescopes, charged with high powers, are capable of great precision. For the remark that a margin of Jupiter, equal to about $\frac{1}{4}$ of the diameter of the satellite, became visible in 45″ of time, adds great support to the accuracy of the observation of the foregoing 4 minutes: and, at all events, it is evidently proved, from the whole of the entrance upon the disc, that the diameter of this satellite is less, by one half at least, than what from the result of the measures of former observers it has been supposed to be.

A method has also been used, of deducing the diameter of the satellites from the time they employ to immerge into the shadow of the planet; but this must be very fallacious, and ought not to be used.

I should not pass unnoticed the apparent magnitude of the satellites. The expressions that have been given of them may be collected into the following narrow compass.

1, 4, 2 4; 1 3–4; 1–2 4, 2, 1 3–4; 1; 2
1, 4, 2 3–2; 1; 4 3–2–1, 4 1½2 4. 1–2
1½2–4 3–2–1, 2 2–1

From which we may conclude, that the 3d satellite is considerably larger than any of the rest; that the 1st is a little larger than the 2d, and nearly of the size of the 4th; and that the 2d is a little smaller than the 1st and 4th, or the smallest of them all.

WM. HERSCHEL.

Slough, near Windsor,
April 30, 1797.

XVI. *Farther Experiments and Observations on the Affections and Properties of Light.* *By* Henry Brougham, *Jun. Esq.* *Communicated by Sir* Charles Blagden, *Knt. Sec. R. S.*

Read June 15, 1797.

HAVING laid before the Royal Society an account of a course of experiments on light, in which I had been engaged, and also of the conclusions which these experiments had taught me to draw, I proceed in the following paper to relate the continuation of my observations; which I hope may not prove wholly uninteresting to such as honoured the former part with their attention. I am first to unfold a new and, I think, curious property of light, that may be indeed reckoned fourfold, as it holds, like the rest, equally with respect to refraction, reflexion, inflexion, and deflexion; thus preserving entire the same beautiful analogy in these four operations, which we have hitherto remarked. I shall then consider several phænomena connected either with this, or with the properties before described, and of which they afford some striking confirmations.

I.

Observation 1. The sun shining strongly into my darkened chamber, I placed, at a small hole in the window-shut, a prism with its refracting angle (of 65°) upwards, so that the spec-

trum was cast on a chart placed at right angles to the incident rays, and four feet from the prism.

In the rays, parallel to the chart, and two feet from it, I placed a pin, whose diameter was $\frac{1}{30}$ of an inch, and fixed it so, that the axis of its shadow on the spectrum might be parallel to the sides of the spectrum. A set of images by reflexion was formed (similar to those described above*), all inclining to the violet; but what I chiefly attended to at present was their shape. I had always observed that the part formed out of the red-making rays was broadest, and that the other parts diminished in breadth regularly towards the violet. I now delineated one or two, at about three inches from the shadow; and though (from the pin's irregularities) the sides were by no means smooth, yet the general shape was in every pin, and with every prism used, nearly as represented in fig. 1. (Tab. X.) divided in the direction R A, according to the colours of the spectrum in which they were formed; R O B A was red, and the broadest; that is, R A was broader than O B, the confines of the red and orange; and G D E V was the violet, narrowest of all.

Observation 2. Between the pin and the prism, $\frac{1}{10}$ of an inch from the pin, was placed a screen, through a small hole in which, of twice the pin's diameter, the rays of the spectrum passed, and were reflected into images by the pin; these were pretty distinct and well defined, when received on a chart $\frac{1}{2}$ a foot from the pin. They were oblong, having parallel sides and confused ends; they were wholly of the colour whose rays fell on the pin, unless when the white, mixed with those at the confines of the yellow and green, caused the images to be of all the

* Phil. Trans. for 1796, page 240.

colours. When the prism was turned round on its axis, so that different rays fell on the pin, the images changed their sizes as well as their positions; they were largest when red, and least when violet.

Observation 3. In case it may be thought that the sides of the hole, through which the rays passed in observation 2, by inflecting, might dispose them, before incidence, into beams of different sizes, I removed the screen, and placed the pin horizontally, the axis of the shadow being now at right angles to that of the prismatic spectrum; and moving the prism on its axis, again I observed the contraction and dilatation of the images by reflexion, though now they were rather less distinct, from the greater size of the incident beam; and to shew that there was both a change of size and of place, without any manner of deception, I placed one leg of a pair of compasses in a fixed point of the spectrum, and the other in the middle point of an image formed by the violet-making rays. The prism being then moved till the image became red, I again bisected it, and found its centre considerably beyond the point of the compasses, which was indeed evidently much nearer one end of the image than the other; besides, that the red image, when measured, was longer than the rest; and this satisfied me that there were two changes, one of place, with respect to the fixed point, the other of size, with respect to the centre of the image. Lastly, as far as I could judge, the dilatation and contraction appeared even and uniform.

Observation 4. I remarked that the fringes or images, by flexion, were always increased in size when formed out of red-making rays, and were less in every other colour, and least in violet (besides being moved farther from the edge of the shadow

in the former rays than in the latter); and this agrees with an observation of Sir Isaac Newton, as far as he tried it, which was with respect to deflexion. In making several experiments with prisms, I hit on a very remarkable confirmation of this. I observed on each side of the spectrum four or five distinct fringes, like the images by reflexion, coloured in the order of the spectrum, but quite well defined at the edge, and even pretty distinct at the end; they were also much narrower than those images, but like them they inclined much to the violet, and were broadest in the red, growing narrower by degrees, and narrowest of all in the violet. I moved the prism and they disappeared, but when the prism was brought back to its former position, they also returned. I then observed the prism in open light, and saw that it had veins, chiefly opaque and white, running through it, and that there were several of these in the place where the light passed when the prism was held as before. But in case the inclination and shape of these images might be owing to the irregular order in which the veins were laid, I held another prism, which happened to have parallel veins; in many positions of this the fringes or images returned, not indeed always so regular nor always of the same kind; for some were confused and broader, formed (as I concluded from this and their position) by reflexion; others made by transparent veins and air-bubbles were also irregular, but inclined to the red, the violet being farthest from the perpendicular, and these were obviously caused by refraction; yet all agreed in this, that they were broadest in the red, and narrowest in the violet parts.

Observation 5. I held, in the direct rays of the sun at $\frac{1}{3}$ an inch from the small hole in the window-shut, a glass tube, free from

scratches and opaque veins, but like most glass that is not finely wrought, having its surface of a structure somewhat fibrous. When this tube was slowly introduced into the light, and so held that none of the rays might be refracted, a streak, chiefly white, was seen, similar in shape and position to those described before.* When narrowly inspected, it was found to contain many images by reflexion in it. But these were much diluted by the abundance of white light, reflected without decomposition in the manner above mentioned.† This streak lay wholly on one side of the tube; but I moved the tube onward a little, and another streak darted through the shadow, and extended all round on both sides: and now, when the tube was in the middle of the rays, there were two streaks on both sides, one a little separated from the other and continued through the shadow, the other on each side of the shadow; the former was evidently produced by refraction; it contained many images very like those by reflexion, only more vivid in the colours, which were all in the inverted order, the violet being outermost, and the rest nearest the point of incidence. Images similar to these are also producible on the retina, as mentioned before.‡

Observation 6. I now placed a prism at the hole, and made the same images by refraction, out of homogeneal light. These inclined to the red, not (like images by reflexion) to the violet; but they were broadest in the red, and grew narrower towards the violet parts. In short, when viewed beside the images by reflexion, except in point of brightness and inclination they differed from them in no respect.

The three first experiments shew, that when homogeneal

* Phil. Trans. 1796, page 236. † Ibid. p. 237. ‡ Ibid. p. 243.

light is reflected, some rays are constantly disposed into larger images than others are, that is, into images more distended in length, though of the same breadth. The fourth experiment shews, that the same takes place when light is inflected and deflected; and the two last shew that the same happens when the rays are refracted in a way similar or analogous to that in which the other images were produced by reflexion and flexion.

We are now to shew, that this difference of size is not owing to the different reflexibilities and flexibilities of the rays. In order to this we shall both demonstrate, and then prove by experience, "that inflexion and deflexion do not decompound "heterogeneous rays, whose direction is such, that they fall on "the bending body." In fig. 2. let A B be the body; G H, E F, C D, the limits of its spheres of deflexion, inflexion, and reflexion, respectively; and let I P be a white ray of direct light entering at P the sphere of deflexion: through P draw L K at right angles to G H; I P will be separated into P R red, and P V violet, and the five other colorific rays according to their deflexibilities; at R and V draw the perpendiculars S T and Q O; then the alternate angles P R T, R P L; and P V Q, V P L are equal each to each. But T R P and Q V P are the angles of incidence, at which the red and violet enter the sphere of inflexion; and R P L, V P L are the angles of deflexion of the red and the violet; therefore the difference of the two latter, that is R P V, is likewise the difference of the two former. Suppose this difference equal to nothing; or that P V and P R are parallel; then rR S the angle of the red's inflexion will be less than vV O the angle of the violet's inflexion, by the angle R P V: (when not evanescent) add R P V to rR S; then rR S will be equal to vV O: that is, the divergence will be destroyed.

and the rays enter the sphere of reflexion, parallel and undecompounded. It is evident, therefore, that the effect arising from the different deflexibilities of the rays is destroyed by the equal and opposite effect produced by their different inflexibilities; and the same thing may in like manner be shewn to happen in the return of the rays from the body after reflexion. But let the rays be so reflected that they shall pass by the body without entering any more than one sphere of flexion; then they will be separated by their flexibilities, as we before described. It appears, then, that if the rays of light were not differently reflexible, flexion could never produce the coloured images, by separating the compound light. And, indeed, this may be easily proved by fact. At 144 feet from the bending body, the greatest fringes by flexion are only half an inch in length, whereas the fourth or fifth images by reflexion are above half an inch at one foot from the reflecting surface: the one sort is therefore more than 144 times more distended than the other, whereas the flexion could, at the very farthest, only double them. Also the distinctness, and brightness, and regularity of the colouring, are quite different in the two cases; the supposed cause would neither account for the order of the colours, nor for their absence in common specular reflexion, and refraction through two prisms joined together with their angles the contrary ways. Lastly, if we suppose the images to be produced by flexion, and then reflected from the body, it would follow that light incident on a prism should be decompounded, formed into several coloured images, and then refracted, the violet being least and the red most bent; all which is perfectly the reverse of what actually happens. I have multiplied the proof of this proposition perhaps beyond what is

necessary; but its great importance to the whole theory will, I hope, plead my excuse.

Let us now suppose that a homogeneal beam passes through the spheres of flexion, it will follow that no divergence can take place from the bending power of the body; so that we have only to estimate the effect produced by the reflexion, and to inquire whether the different reflexibilities of the rays can cause the images to vary their sizes according as they are formed by different rays. In fig. 8. let A B be the body, C D the limit of its sphere of reflexion, and I P a beam of homogeneal rays, as red, incident at P and reflected to R, forming there the image R*r*. It is evident that the greater reflexibility of the rays I P can only alter the position of the centre of R*r*, making it nearer the perpendicular than the centre of an image formed by any other rays would be. But the greater length of R*r* shews that a greater quantity of rays is reflected, or that the same quantity is spread over a greater space, and that in the following way. Let I F *fi* be a beam of violet-making rays entering A B C D, and reflected so as to form the image R*v*. The force exerted by A B decreasing according to some law (of which we are as yet ignorant) as the distance increases, is not sufficient to turn the rays back till they have come a certain length within A B C D. But for the same reason it turns back all that it does reflect before they come nearer than a certain distance; between these two limits, therefore, the rays are turned back. But the limits are not the same to all the rays; some begin to be turned at a greater distance from the body than others, and consequently are reflected to a greater distance from the middle ray of the incident beam. Thus if I F *fi* be changed to a red-making beam, it begins to be turned back

at *f*, and the rays farthest from AB are reflected to *r* instead of to *v*, where they fell when I F *fi* was violet-making; not but that the same quantity of rays is reflected, the only difference is, that the most reflexible are reflected farthest from the body by their greater reflexibility, and farthest from each other by this other property. Exactly the same happens in the case of refraction, *mutatis mutandis*; but there seems to be a slight variation in the *manner* in which the different rays are disposed into images of different sizes by flexion. In this case also the bending body's action reaches farther when exerted on some rays than when exerted on others: but then, the direction of the rays not passing through the body, those which are farthest off and at too great a distance to be bent, never coming nearer, are not bent at all; and consequently as the least flexible rays are in this predicament at the smallest distance, and the most flexible not till the distance is greater, the images formed out of the former must be less than those formed out of the latter. This difference in the way in which the phænomenon appears, does not argue the smallest difference in the cause; it only follows from the different position of the rays, with respect to the acting body, in the two cases. I infer then from the whole, that different sorts of rays come within the spheres of flexion, reflexion, and refraction, at different distances, and that the actions of bodies extend farthest when exerted on the most flexible. It may perhaps be consistent with accuracy and convenience to give a name to this property of light; we may, therefore, say that the rays of light differ in degree of refrangity, reflexity, and flexity, comprehending inflexity and deflexity. From these terms (uncouth as, like all new words, they at first appear) no confusion can arise, if we always re-

member that they allude to the degree of distance to which the rays are subject to the action of bodies. I shall only add an illustration of this property, which may tend to convey a clearer idea of its nature. Suppose a magnet to be placed so that it may attract from their course a stream of iron particles, and let this stream pass at such a distance that part of it may not be affected at all; those particles which are attracted may be conceived to strike on a white body placed beyond the magnet, and to make a mark there of a size proportional to their number. Let now another equal stream considerably adulterated by carbonaceous matter, oxygene, &c. pass by at the same distance, and in the same direction. Part of this will also be attracted, but not so far from its course, nor will an equal number be affected at all; so that the mark made on the white body will be nearer the direction of the stream, and of less size than that made by the pure iron. It matters not whether all this would actually happen, even allowing we could place the subjects in the situation described; the thing may easily be conceived, and affords a good enough illustration of what happens in the case of light.

Pursuant to the plan I before followed, I now tried to measure the different degrees of reflexity, &c. of the different rays; but though the measurements which I took agreed in this, that the red images were much larger than the rest, and the green appeared by them of a middle size, yet they did not agree well enough (from the roughness of the images, and several other causes of error), to authorize us to conclude with any certainty "that the action of bodies on the rays is in proportion to the "relative sizes of these rays." This, however, will most pro-

bably be afterwards found to be the case; in the mean time there is little doubt that the sizes are the cause of the fact.

II.

Several phænomena are easily explicable on the principles just now laid down.

1. If a pin, hair, thread, &c. be held in the rays of the sun refracted through a prism, extending through all the seven colours, a very singular deception takes place: the body appears of different sizes, being largest in the red and decreasing gradually towards the violet. This appearance seemed so extraordinary, that some friends who happened to see it as well as myself, suspected the body must be irregular in its shape. On inverting it, however, the same thing took place; and on turning the prism on its axis, so that the different rays successively fell on the same parts, the visible magnitude of the body varied with the rays that illuminated it. This appearance is readily accounted for by the different reflexity of the rays, and follows immediately from Obs. 2. and 3.

2. Sir ISAAC NEWTON found that the rings of colours made by thin plates and by thick plates of glass (as he calls them), when formed of homogeneal light, varied in size with the rays that made them, being largest in the most flexible rays. I have had the pleasure of observing several other sorts of rings, so extremely similar, and formed by flexion, that I can no longer doubt of this being also the cause of the phænomena observed by NEWTON. I shall first describe a species, to prove "that the colours by thick and thin plates are one and the "same phænomenon, only differing in the thickness of the

"plates." Happening to look by candle-light upon a round concave plate of brass, pretty well polished, so as to reflect light enough for shewing an image of the candle, I was surprised to see that image surrounded by several waves of colours, red, green, and blue, disposed in pretty regular order. This was so uncommon in a metallic speculum, that I examined the thing very minutely by a variety of experiments; these I shall not particularly now describe, but give a general idea of their results.

It must be observed, for the sake of clearness, that in the following inquiries concerning the formation of rings or fringes, the diameter of a ring or fringe means the line passing through the centre of that ring, and terminated at both ends by the circumference; whereas the breadth means that part of the diameter intercepted between the limits of the ring, or the distance between its extreme colours, red and violet.

In the first place, they were formed by the sun's light in the figure of rings surrounding the centre of the sphere to which the plate was ground, at greater distances increasing their breadths, the colours pretty bright, though inferior in brilliancy to those of concave specula.

Secondly, the order of the colours was in all red outermost, and violet or blue innermost, with a greyish-blue spot in the common centre of the whole; and on moving the plate from the perpendicular position, the rings moved and broke exactly like those of specula.

In the third place, homogeneal light made them of simple colours; they were broadest when red, narrowest when blue and violet.

Fourthly, they decreased in breadth from the centre; and I

found, by a simple contrivance, that they were to one another in the very same ratio that the rays by specula follow.

In the fifth place, I compared the general appearance of the two sorts by viewing them at the same time, and was struck with their general appearance, unless that these of specula were most vivid and distinct.

These things made me suspect that they were actually caused by the thin coat of gums with which the surface of the plate was varnished, called lacker. Accordingly I took it off with spirit of wine, and found the rings disappear; on lackering it again they returned; and in like manner I caused a well finished concave metal speculum to form the rings of which we are speaking, by giving it a thin coat of lacker. This is a clear proof that these rings were exactly the same with those of thick plates (to use NEWTON'S expression), for the coat of gums is, when thin, pretty transparent, as may be seen by laying one on glass plates.

But this coat is extremely thin, and cannot exceed the 200th part of an inch; so that the colours of thick plates are in fact the very same with those of thin plates, except that the two kinds are made by different sized plates. We cannot, therefore, distinguish them, any more than we do the spectrum made by a prism whose angle is 90° from that made by one whose angle is 20°. This kind of colours is not the only one I have observed of nearly the same kind with those of plates; we shall presently see another much more curious and remarkable.

III.

In reflecting on the observations and conclusions contained in my former paper, several consequences seemed to follow,

which appeared so new and uncommon, that I began to doubt a little the truth of the premises; but at any rate was resolved to examine more minutely how far these inferences might be consistent with fact: and I am happy in being able to announce the completeness of that consistency, even beyond my expectations. The chief consequences were the following.

1. That a speculum should produce, by flexion and reflexion, colours in its reflected light wherever it has the least scratch or imperfection on its surface.

2. That on great inclinations to the incident rays all specula, however pure and highly polished, should produce colours by flexion.

3. That they should also in the same case produce colours by reflexion.

4. That lenses, having the smallest imperfections, should produce by flexion colours in their refracted light.

5. That there should be many more than three, or even four fringes by flexion, invisible to the naked eye. And,

6. That Iceland crystal should have some peculiarities with respect to flexion and reflexion; or if not, that some information should be acquired concerning its singular properties respecting refraction.

The manner in which the first of these propositions is demonstrated *a priori*, is evident from the 4th figure, where C D is the reflecting surface, *v o* a concavity bearing a small ratio to C D, A *o* and A B rays proceeding to C D. The one, A B, will be separated into B *r* red, and B *v* violet, by deflexion from *o*, and will be reflected to *r' v'*, forming there the fringes. The other, A *o*, being reflected, will be separated into B *x* and B *y*, by deflexion from *v*, forming other fringes, *x y*, on the

side of $v\,o$'s shadow opposite to $r'\,v'$. Also when $v\,o$ is convex instead of concave, the like fringes will be produced by the rays being deflected in passing by its sides. Lastly, when $v\,o$ is a polished streak, images by reflexion will be produced, as described Phil. Trans. for 1796, p. 269. The same passage will also shew the reason why, on great inclinations, colours by reflexion should be produced. And the second proposition, with respect to flexion, follows from what was demonstrated in this paper (p. 357 and 358); it being that case where the rays either leave or fall on the speculum at such an inclination, as to come only within the sphere of inflexion, without being deflected. The fourth proposition is merely a simple case of flexion. And the two last require no illustration. I shall now relate how I inquired into the truth of these things *a posteriori*.

Observation 1. Looking at a plane glass mirror exposed to the sun's light, I observed that up and down its surface there were minute scratches (called hairs by workmen), and that each of these reflected a bright colour, some red, others green, and others blue. On moving the mirror to a different inclination, or my eye to a different position with respect to the mirror, I saw the species of the colours change; the red, for instance, became green, and the green blue. I applied my eye close to the mirror, and received on it the light reflected from one hair. I observed several distinct images of the sun much distended and regularly coloured, just like those described above; the same appearances were observable in all specula, metal and glass, which had these hairs, and I never saw any metal one without some: their size is exceedingly small, not above $\frac{1}{1000}$ of an inch. Rubbing a minute particle of grease on the surface of the speculum, images were seen on the fibrous surface;

and they always lay at right angles to that direction in which the grease was disposed by drawing the hand along it.

Observation 2. Besides these polished hairs, many specula have fewer or more small specks and threads, rough and black. Perhaps every polished surface is studded with a number of small ones, invisible to the naked eye from the quantity of regular light which it reflects. I took, from a reflecting telescope, a small concave speculum not very well finished; its surface shewed several specks to the naked eye, and many with a microscope. Its diameter was $\frac{1}{10}$ of an inch, its focal distance two inches, and the sphere to which it was ground eight inches diameter. I placed it at right angles to the rays of the sun, coming through a small hole $\frac{1}{42}$ of an inch diameter, into a very well darkened room; I then moved it vertically, so that the rays might be reflected to a chart 12 inches from the speculum, and consequently 10 from the focus: and though the focus appeared white and bright, yet on the chart the broad image was very different. It was mottled with a vast number of dark spots; these were of two sorts chiefly, circular and oblong. Of the former a considerable number were distinct and large, the rest smaller and more confused, but so numerous that they seemed to fill the whole image. None were quite black, but rather of a bluish grey, and the oblong ones had a line of faint light in the middle, just as is the case in shadows of small bodies. But the chief thing which I remarked was the colours. Each oblong and round spot was bordered by a gleam of white, and several coloured fringes separated by small dark spaces. The fringes were exactly like those surrounding the shadows of bodies, of the same shape with the dark space, having the colours in the order, red on the outside, blue or violet in the inside; the in-

nermost fringe was broadest, the others decreasing in order from the first. I could sometimes see four of them, and when made at the edge of the large image, I could indistinctly discern the lineaments of a fifth: when two of the spots were very near one another, their rings or fringes ran into one another, crossing.

Observation 3. When the chart was removed to a greater distance, as six feet, the fringes were very distinct and large in proportion; also the smaller spots became more plain, and their rings were seen, though confusedly, from mixing with one another. When the speculum was turned round horizontally, so that its inclination to the incident rays might be greater, the distance of the chart remaining the same (by being drawn round in a circle), the spots and fringes evidently were distended in breadth. I have endeavoured to exhibit the sun's image, as mottled with fringes or rings and spots, in fig. 5.

Observation 4. I placed the speculum behind a screen with a hole in it, through which were let pass the homogeneal rays of the sun, separated by refraction through a prism; this being turned on its axis, the rays which fell on the speculum were changed; the fringes were now of that colour whose rays fell, and when the rays shifted, the fringes contracted or dilated, being broadest in the most flexible rays, and consequently in those whose flexity is greatest.

Observation 5. The direct light falling on the speculum, and part of the reflected light on the horizontal white stage of a very accurate micrometer, I measured the breadth of the fringes, spots, &c. These, with the distance of the speculum from the window and micrometer, and the size of the sun's image, are set down in the following table, all reduced to inches and decimals.

	Inches.	Parts.
Distance of the speculum from the hole in the window-shut	24.	
Distance of the speculum from the stage of the micrometer	18.	
Transverse axis of the sun's image	2.	6
Conjugate axis of the sun's image	1.	4
Length of the oblong dark spot		.4
Breadth of the oblong dark spot		.0074
Breadth of its first fringe		.0022
Elliptic spot's transverse axis		.0116
——— conjugate axis		.0068
Breadth of its first fringe		.0034
Transverse axis of a larger elliptic spot		.013
Conjugate axis of the same spot		.0076

In the image where these measures were taken, there were seven other elliptic spots, a little less and nearly equal; all the others were much smaller and more confused.

Observation 6. On viewing the surface of the speculum attentively in that place whence the rays formed the oblong and first mentioned elliptic spots, I saw a dark but very thin long scratch, and a dark dent, similar in shape to the dark spaces on the image; the dark spot measured less than $\frac{1}{250}$ of an inch; which makes its whole surface to the whole polished surface as 1 to 34225, supposing the former circular or nearly so. All these measures will be found to agree very well, for their smallness and delicacy: thus, the ratio last mentioned is nearly the same which we obtain by comparing the image and the spot; the like may be said of the two spots mentioned in the table,

i. e. their axes are proportional. I now could produce what spots I pleased, by gently scratching the speculum, or by making lines, dots, &c. with ink, and allowing it to dry; for these last formed convex fibres, which produced coloured fringes as well as the concavities, agreeably to what was deduced *a priori*.

Observation 7. The whole appearance which I have been describing bore such a close and complete resemblance to the fringes made round the shadows of bodies, that the identity of the cause in both cases could not be doubted. In order however to shew it still further, I measured the breadths of two contiguous fringes in several different sets; the measurements agreed very well, and gave the breadth of the first fringe .0056, and of the second .0034; or of the first .0066, and of the second .0034. The ratio of the breadths by the first is 28 to 17; by the second 30 to 17; of which the medium is 29 to 17, and this is precisely the ratio of the two innermost fringes made by a hair, according to Sir ISAAC NEWTON's measurement: the first being, according to him, $\frac{1}{170}$ of an inch; the second $\frac{1}{290}$ of an inch.* Farther, the two innermost rings made by plates have their diameters (not breadths) in the ratio of $1\frac{1}{16}$ to $2\frac{1}{8}$†, and the distance between the middle of the innermost fringes (made by a hair), on either side the shadow, is to the same distance in the second fringes as $\frac{1}{38}$ to $\frac{3}{47}$; therefore the diameters of the two first rings made by the specks in the speculum, are as $\frac{302}{685}$ to $\frac{437}{1365}$; which ratio differs exceedingly little from that of $1\frac{1}{16}$ to $2\frac{1}{8}$, the ratio of the diameters of rings made by plates, either those called by NEWTON thick, or those which

* Optics, Book 3. Obs. 3. † Book 2. Parts 1 and 4.

he names thin: for suppose this difference nothing, $2\frac{1}{6} \times \frac{202}{663} = 1\frac{211}{1326} \times \frac{627}{1363}$; and the difference between these two products (now stated equal) is not much above $\frac{1}{8}$ in reality.

Observation 8. The last thing worth mentioning in these phænomena was this: I viewed the fringes through a prism, holding the refracting angle upwards, and the axis parallel to that of the dark space; then moving it till the objects ceased descending, I saw in that posture the fringes much more distinct and numerous; for I could now see five with ease, and several more less distinctly. This led me to try more minutely the truth of the 5th proposition, with respect to the number of the fringes surrounding the shadows of bodies in direct light. Having produced a bright set of these by a blackened pin $\frac{1}{24}$ of an inch in diameter, I viewed them through a well made prism, whose refracting angle was only 30°, and held this angle upwards, when the fringes were on the side of the shadow opposite to me; I then moved the prism round on its axis, and when it was in the posture between the ascent and descent of the objects, I was much pleased to see five fringes plainly, and a great number beyond, decreasing in size and brightness till they became too small and confused for sight. In like manner those formed by a double flexion of two bodies, and those made out of homogeneal light, were seen to a much greater number when carefully viewed through the prism. And this experiment I also tried with all the species of fringes by flexion which I could think of.

Observation 9. The same appearances which were occasioned by the metal speculum, might be naturally expected to appear when a glass one was used. But I also found the like rings or fringes of colours and spots in the image beyond the focus of

a lens; nor was a very excellent one belonging to a DOLLOND'S telescope free from them. The rings with their dark intervals resembled those floating specks so often observed on the surface of the eye, and called "*muscæ volitantes*," only that the muscæ are transparent in the middle, because formed by drops of humor: they will, however, be found to be compassed by rings of faint colours, which will become exceedingly vivid if the eyes be shut and slowly opened in the sun's light, so that the humor may be collected; they also appear by reflexion, mixed with the colours described in Phil. Trans. for 1796, p. 268.

Observation 10. The sun shining strongly on the concave metal speculum, placed at such a distance from the hole in the window that it was wholly covered with the light; upon inclining it a little, the image on the chart was bordered on the inside with three fringes similar to those already described; on increasing the inclination these were distended, becoming very bright and beautiful; when the inclination was great, and when it was still increased, another set of colours emerged from the side next the speculum, and was concave to that side. Here I stopped the motion, and the image on both sides of the focus had three sets of fringes, and four fringes in each set; but when viewed through a prism (as before described), the numbers greatly increased, both the fringes and the dark intervals decreasing regularly. The appearance to the naked eye is represented in fig. 6. where A D C being the image, A and C are the sets of fringes at the edges, and B the third set, there being none at E and D the sides, since the light which illuminates these quarters comes not from the edges of the speculum in so great inclinations. I now viewed the surface of the speculum, and saw it, in the place answering to B in the image, covered with

fringes exactly corresponding with those at B; and on changing the figure of that part of the speculum's edge between them and the sun, the fringes likewise had their figure altered in the very same way. On moving the speculum farther round, B came nearer to A in the image, according as the fringes on the speculum receded from that side which formed them; and before they vanished alike from the speculum and image, they mixed with the colours at A in the image, and formed in their motion a variety of new and beautiful compound colours; among these I particularly remarked a brown chocolate colour, and various other shades and tinges of brown and purple. Just before the fringes at B appeared, the space between A and C was filled with colours by reflexion, totally different in appearance from the fringes; but I could not examine them so minutely as I wished in this broad image, I therefore made the following experiment.

Observation 11. At the hole in the window-shut I held the speculum, and moved it to such an inclination that the colours by reflexion might be formed in the image; they were much brighter and far more distended than the fringes, and were in every respect like the images by reflexion in the common way, only that the colours were a little better and more regular. They were also seen on the speculum as the third set of fringes had before been in *Obs.* 10.; but by letting the rays fall on the half next the chart, and inclining that half very much, I could produce them, though less distinctly, by a single reflexion. I now held a plain metal speculum so that the rays might be reflected to form a white image on a chart. On inclining the speculum much, I saw the image turn red at the edge; it then became a little distended; and lastly, fringes

emerged from it well coloured, and in regular order, with their dark intervals. This may easily be tried by candle-light with a piece of looking-glass, and those who without much trouble would satisfy themselves of the truth of the whole experiment contained in this and the last observation, may easily do it in this way with a concave speculum; but the beauty of the appearance is hereby quite impaired. After this detail it is almost superfluous to add, that the fringes at B, fig. 6. are formed by deflexion from the edge of the speculum next the sun, and then falling on it are reflected to the chart; that the images by reflexion are either formed by the light being decompounded at its first reflexion, and then undergoing a second, or, in other instances, without this second reflexion; and that the other fringes are produced exactly as described above, from the necessary consequences of the theory. I shall only add, that nothing could have been more pleasing to me than the success of this experiment; not only because in itself it was really beautiful from its variety, but also because it was the most peremptory confirmation of what followed from the theory *a priori*, and in that point where the singularity of its consequences most inclined me to doubt its truth.

Let us now attend to several conclusions to which the foregoing observations lead, independently of the propositions (*viz.* the five first) which they were made to examine.

I. We must be immediately struck with the extreme resemblance between the rings surrounding the black spots on the image made by an ill polished speculum, and those produced by thin plates observed by NEWTON; but perhaps the resemblance is still more conspicuous in the colours surrounding the image made by any speculum whatever, and fully described in

Obs. 10. and 11. The only difference in the circumstances is now to be reconciled. The rings surrounding the black spot on the top of a bubble of water, and those also surrounding the spot between two object glasses,* have dark intervals (exactly like those rings I have just now described, and the fringes surrounding the shadows of bodies); but these intervals transmit other fringes of the same nature, though with colours in the reverse order; from which Sir ISAAC NEWTON justly inferred, that at one thickness of a plate the rays were transmitted in rings, and at another reflected in like rings. Now it is evident, that neither reflexibility nor refrangibility will account for either sort of rings, because the plate is far too thin for separating the rays by the latter, and because the colours are in the wrong order for the former; and also because the whole appearance is totally unlike any that refrangibility and reflexibility ever produce. To say that they are formed by the thickness of the plates, is not explaining the thing at all. It is demanded in what way? and indeed we see the like dark intervals and the same fringes formed at a distance from bodies by flexion, where there is no plate through which the rays pass. The state of the case then seems to be this: "when a "phænomenon is produced in a particular combination of cir- "cumstances, and the same phænomenon is also produced in "another combination, where some of the circumstances, be- "fore present, are wanting; we are intitled to conclude that "the latter is the most general case, and must try to resolve "the other into it." In the first place, the order of the colours in the NEWTONIAN rings is just such as flexion would produce; that is, those which are transmitted have the red inner-

* Optics, Book II. P. 1.

most, those which are reflected have the red outermost; the former are the colours arranged as they would be by inflexion, the latter as they would be by deflexion; and here by outermost and innermost must be understood relative position only, or position with respect to the thickness of the plate, not of the central spot. Secondly, the thinnest plate makes the broadest ring (the diameter of the rings being in the inverse subduplicate ratio of the plate's thickness); just so is it with fringes by flexion; nearer the body the fringes are broadest, and their diameters increase in the same ratio with the diameters of the rings by plates whose thickness is uniform; each distance from the bending body therefore corresponds with a ring or fringe of a particular breadth, and the alternate distances correspond with the dark intervals: the question then is, what becomes of the light which falls on or passes at these alternate distances? In the case of thin plates, this light is transmitted in other rings; we should therefore be led to think that in the case of the light passing by bodies, it should be at one distance inflected, and at another deflected; and in fact the phænomena agree with this, for fringes are formed by inflexion within the shadows of bodies; they are separated by dark intervals; the fringes and the intervals without the shadow decrease in breadth according to the same law; so that the fringes and intervals within the shadow correspond with the intervals and fringes without, respectively. Nor will this explanation at all affect the theory formerly laid down; it will only (if found consistent with farther induction) change the definite spheres of inflexion and deflexion into alternate spheres. At any rate, the facts here being the same with those described by NEWTON, but in different circumstances, teach us to reconcile the difference, which

we have attempted to do, as far as is consistent with strictness; and what we have seen not only entitles us to conclude that the cause is the same, but also inclines us to look for farther light concerning that cause's general operation: and I trust some experiments which I have planned, with an instrument contrived for the purpose of investigating the ratio of the bending power to the distances at which it acts, will finally settle this point.

II. Another conclusion follows from the experiments now related, *viz.* that we see the great importance of having specula for reflectors delicately polished; not only because the more dark imperfections there are on the surface, the more light is lost, and the more colours are produced by flexion (these colours would be mostly mixed and form white in the focus), but also because the smallest scratches or hairs, being polished, produce colours by reflexion, and these diverging irregularly from the point of incidence are never collected into a focus, but tend to confuse the image. Indeed it is wonderful that reflectors do not suffer more from this cause, considering the almost impossibility of avoiding the hairs we speak of: however, that they do actually suffer is proved by experience. I have tried several specula from reflecting telescopes, and found that though they performed very well, from having a good figure, yet from the focus (when they were held in the sun's light) several streaks diverged, and were never corrected; others had the hairs so small, that it was very difficult to perceive the colours produced by them, unless they fell on the eye. Glass concaves were freer from these hairs, but they were much more hurt by dark spots, &c. In general the hairs are so small in well wrought metals, that they do little hurt; but when en-

larged by any length of exposure to the light and heat in solar observations, they produce irregularities round the image. Such at least I take to be the explanation of the phænomenon, observed at Paris by M. DE BARROS during the transit of Mercury in 1743, and recorded in Phil. Trans. for 1758. But there is another more serious impediment to the performance of reflectors, and which it is to be feared we have no means of removing. In making the experiments of which the history has been given, on viewing attentively the surface of the speculum, every part of it was seen covered with points of colours, formed by reflexion from the small specular particles of the body. I never saw a speculum free in the least from these, so that the image formed in the focus must be rendered much more dim and confused by them, than it otherwise would be.

III. The last conclusion which may be drawn from these experiments, is a very clear demonstration in confirmation of what was otherwise shewn, concerning the difference between coloured images produced by reflexion, and those made by flexion. This complete diversity is most evident in the experiments with specula, the colours produced by which, in the form of fringes and rings, ought, as well as the others described as images by reflexion in *Obs.* 11, to be the same in appearance with those formed by pins; whereas no two things can be more dissimilar.

It remains to examine the 6th proposition: for this purpose I made the following observations.

Observation 1. Having procured a good specimen of Iceland crystal, I split it into several pieces, and chose one whose surface was best polished. I exposed this to a small cone of the sun's light, and received the reflected rays on a chart; nothing

was observable in the image, farther than what happens in reflexion from any other polished body. Some pieces, indeed, doubled and tripled the image, but only such as were rough on the surface, and consequently presented several surfaces to the rays. When smooth and well polished, a single image was all that they formed. The same happened if I viewed a candle, the letters of a book, &c. by reflexion from the Iceland crystal.

Observation 2. I ground a small piece of Iceland crystal round at the edge, and gave it a tolerable polish here and there by rubbing it on looking-glass, and sometimes by a burnisher (it would have been next to impossible to polish it completely). I then placed the polished part in the rays near the hole in the window-shut, and saw the chart illuminated with a great variety of colours by reflexion, irregularly scattered, as described above;* I therefore held the edge in the smoke of a candle and blackened it all over, then rubbed off a very little of the soot, and exposed it again in the rays. I now got a pretty good streak of images by reflexion, in no respect differing from those made in the common way. Nor could I ever produce a double set, or a single set of double images, by any specimen properly prepared, either on a chart by the rays of the sun, or on my eye by those of a candle.

Observation 3. I ground to an even and pretty sharp edge two pieces of Iceland crystal, and placed one in the sun's rays. At some feet distance I viewed the fringes with which its shadow was surrounded, and saw the usual number in the usual order. I then applied the other edge so near that their spheres of flexion might interfere in the manner before described,† and

* Phil. Trans. for 1796, p. 270. † Ibid. p. 256.

thus the fringes might be distended; still no uncommon appearance took place; nor when other bodies were used with one edge of crystal, nor when polished pieces of different shapes and sizes were employed. The same things happened by candle light, and also by refracted homogeneal light. In short, I repeated most of my experiments on flexion with Iceland crystal, and found that they were not changed at all in their results.

Observation 4. Having great reason to doubt the accuracy of an experiment tried by Mr. MARTIN, and in which, by a prism of Iceland crystal, he thought six spectra were produced, I was not much surprised to find, that a prism made by polishing the two contiguous sides of a parallelopiped of Iceland crystal produced only two equal and parallel images, in whatever position the prism was held. But though, from the imperfect account which MARTIN gives of this appearance, it was impossible to discover his error from his own words, yet chance led me to find out what most probably had misled him; for looking at a candle through the opposite sides of a specimen of Iceland crystal, I saw four coloured images (besides two white ones) of the candle. These were parallel to one another, and in the same line, as represented in fig. 7. where E represents the two regular images, G and F two others coloured very irregularly, and changing colours as the crystal was moved horizontally, sometimes appearing each two-fold, and its two parts of the same or different colours. A and B were regularly coloured, and evidently formed by refraction, and reflected back from the sides. On turning the crystal round, so that its position might be at right angles to its former position, the images moved round, and were in a line perpendicular to AB, as CD. All this happened in like manner in the sun's rays; and on

viewing the specimen, I found it was split and broken in the inside, so as to be lamellated in directions parallel, or nearly so, to the sides; on these plates there were colours in the day time by the light of the clouds: and it is evident that it was these fractures which caused the irregular images G and F, for other specimens shewed no such appearance. I would therefore conclude, that Iceland crystal separates the rays of light into two equal and similar beams by refraction, and no more.*

As to the cause of the separation, I would hope that some information may be obtained from the experiments I have related: for from them it appears, that this singular property extends no farther than to the action of the particles of Iceland crystal on the particles of light in their passage through the body; and from *Obs.* 4. it is farther evident, that it is not owing to the different properties which Sir ISAAC NEWTON conjectures the different sides of rays to have; for if this were the cause, when the rays pass between two pieces of crystal, an uncommon flexion would take place. Lastly, another fact (mis-stated by BARTOLIN † and ROMÉ DE LISLE) ‡ shews, that the unusual refraction takes place within the body, while the

* Mentioning this account of MARTIN's mistake to Professor ROBISON, of this university, I was pleased to find a full confirmation of it. It was that excellent philosopher who shewed the appearance to MARTIN; but he not understanding it, took the liberty of publishing the observation as his own, after first mangling it in such a way as to give him, indeed, some pretext for the appropriation. The Professor merely mentioned his having communicated it to Mr. MARTIN; how the latter used it we have shewn in the text: the theory of the appearance is somewhat more complex than appears by my observations. I was therefore pleased to find that the Professor was in possession of the true account of it; which is, however, foreign to the present purpose.

† *Experimenta Crystalli*, abridged in Phil. Trans. Vol. V.

‡ *Cristallographie*, Vol. I.

other, like all refractions, begins at some small distance before the rays enter.

The writers just now quoted assert, that if the crystal be turned round so as to assume different positions, there is one in which the line appears single. The fact is very different, as follows. When the crystal is turned round, the unusual image moves round also, and appears above the other; the greatest distance between the two images is when they are parallel to the line bisecting one of the acute angles of the parallelogram through which the rays pass; when the images are parallel to a line bisecting one of the obtuse angles they seem to coincide; but they will be found, if observed more nearly, to coincide only in part. Thus (in fig. 9.) A B and C D are the two black lines at their greater distance, and their extremities A and C, B and D are even with one another; that is, the figure formed by joining A and C, B and D is a rectangle. But in the other case (fig. 8.) A B and C D being the lines, the space C B (equal in depth of colour to the real line on the paper), is the only place in which the lines (or images) coincide. The space A C of A B, and B D of C D are still of a light colour, and the two lines A B and C D do not coincide, by the difference A C or B D; that is, by the difference O P, the greatest distance (fig. 9.). In short, the unusual line's extremities describe circles (in the motion of the crystal) whose centres are the extremities of the usual line, and whose radii are the greatest distance. From this it appears evident, that the unusual image is formed within the crystal, and turns round with the side of the particle, or rhomboidal mass of particles, which forms it. Farther, it is evident that the power which produces the division of the incident light, is very different from common refraction, from the motion, and

the effect taking place when the rays are perpendicular. Suspecting, therefore, that it might be owing to flexion, I made the following experiment, which undeceived me.

Observation 5. I covered one side of a specimen of Iceland crystal, three inches deep, with black paper, all but a small space $\frac{1}{50}$ of an inch in diameter, and placed a screen with a hole of the same size, six feet from the hole in the window-shut of my darkened chamber, so that the rays might pass through the screen, and fall on a prism placed behind, to refract them into a small and well defined spectrum, which was received on a chart two feet from the prism. This spectrum I viewed through the crystal, and of course saw it doubled; but the two images were by no means parallel; the unusual one inclined to the red, and its violet was considerably farther removed from the violet of the other, than the two reds were from one another; which shews, that the most refrangible or least flexible rays were farthest moved from their course by the unusual action, and proves this to be very different from flexion.*

From all these observations this conclusion follows; that the remarkable phænomenon in question arises from an action very different from either refraction or flexion; and whose nature well deserves to be farther considered. It may possibly belong to the particles of Iceland crystal, and in a degree to those of rock crystal, from the form and angles of the rhomboidal masses, whereof these bodies are composed. Nor is this conjecture at all disproved by the fact that glass shaped like these bodies wants the property; for we cannot mould the *particles* of glass, we can only shape large *masses* of these; whereas we

* When a candle or line is viewed through a deep specimen, the unusual image is tinged with colours.

cannot doubt that in crystallisation the smallest masses assume the same form with the largest: but then other hypotheses may perhaps also account for the fact, such as atmospheres, electric fluid, &c. &c.; so that till farther observations are made we ought to rest contented with barely suggesting the query. In the mean time, reserving to a future opportunity some inquiries concerning the chemical properties of light, and the nature of the forces which bodies exert on it internally, I conclude at present with a short summary of propositions. But first, may I be permitted to express a hope, that what has been already attempted (and for which no praise can be claimed farther than what is due to attentive observation, according to the rules of the immortal BACON), may prove acceptable to such as love to admire the beautiful regularity of nature, or more particularly to trace her operations [illegible] one of the most pleasing, most important, and most [illegible] walks of physical science.

Proposition I. The sun's light consists of rays which differ in degree of refrangity, reflexity, inflexity, and flexity; and the rays which are most flexible have also the greatest refrangity, reflexity, and flexity; or are most *refrangile*, *reflexile*, and *flexile*.

Proposition II. Rays of compound light passing through the spheres of action and falling on the bending body, are not separated by their flexibility, either in their approach to, or return from the body.

Proposition III. The colours of thin and those of thick plates are precisely of the same nature, differing only in the thickness of the plate which forms them.

Proposition IV. The colours of plates are caused by flexion, and may be produced without any transmission whatever.

Fig. 1.

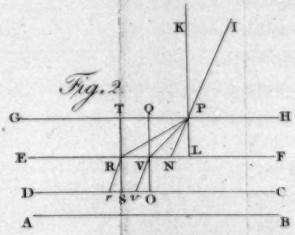


Fig. 2.

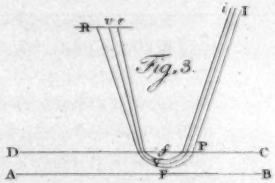


Fig. 3.

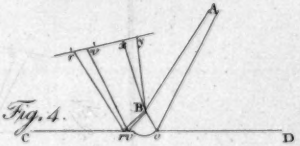


Fig. 4.

Fig. 5.

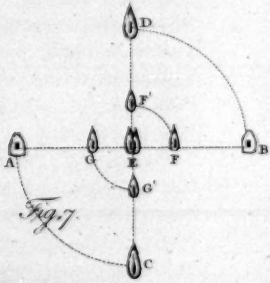


Fig. 7.

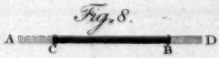


Fig. 8.

Fig. 9.

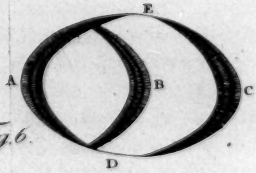


Fig. 6.

Ruyton Sc.

Proposition V. All the consequences deducible from the theory *a priori* are found to follow in fact.

Proposition VI. The common fringes by flexion (called hitherto the "*three fringes*"), are found to be as numerous as the others.

Proposition VII. The unusual image by Iceland crystal is caused by some power inherent in its particles, different from refraction, reflexion, and flexion.

Proposition VIII. This power resembles refraction in its degree of action on different rays; but it resembles flexion within the body, in not taking place at a distance from it, in acting as well on perpendicular as on oblique rays, and in its sphere or space of exertion moving with the particles which it attends.

XVII. *On Gouty and Urinary Concretions.* By William Hyde Wollaston, *M. D. F. R. S.*

Read June 22, 1797.

If in any case a chemical knowledge of the effects of diseases will assist us in the cure of them, in none does it seem more likely to be of service than in the removal of the several concretions that are formed in various parts of the body. Of these one species from the bladder has been thoroughly examined by Scheele, who found it to consist almost entirely of a peculiar concrete acid, which, since his time, has received the name of lithic.

In the following paper I purpose giving an account of the analysis of gouty concretions, and of four new urinary calculi.

The gouty matter, from its appearance, was originally considered as chalk; but from being found in an animal not known to contain or secrete calcareous earth uncombined with phosphoric acid, it has since been supposed to resemble earth of bones. Dr. Cullen has even asserted, that it is 'very entirely' soluble in acids. The assertion, however, is by no means generally true, and I think he must, in all probability, have used the nitrous acid, for I find no other that will dissolve it.

Another opinion, and, I believe, at this time the most prevalent is, that it consists of lithic acid, or matter of the calculus described by Scheele. But this idea is not, I believe, founded

on any direct experiments, nor is it (to my knowledge) more ably supported than by Mr. FORBES, who defends it solely by pathological arguments from the history of the disease. Had he undertaken an examination of the substance itself, he would have found that, instead of a mere concrete acid, the gouty matter is a neutral compound, consisting of lithic acid and mineral alkali; as the following experiments will prove.

(1.) If a small quantity of diluted vitriolic acid be poured upon the chalk-stone, part of the alkali is extracted, and crystals of GLAUBER's salt may be obtained from the solution. Common salt may still more easily be procured by marine acid. The addition of more acid will extract the whole of the alkali, leaving a large proportion of the chalk-stone undissolved; which exhibits the following characteristic properties of lithic matter.

(*a.*) By distillation it yields a little volatile alkali, Prussic acid, and an acid sublimate, having the same crystalline form as the sublimate observed by SCHEELE.

(*b.*) Dissolved in a small quantity of diluted nitrous acid it tinges the skin with a rose colour, and when evaporated leaves a rose-coloured deliquescent residuum.

(*c.*) It dissolves readily in caustic vegetable alkali, and may be precipitated from it by any acid, and also by mild volatile alkali; first as a jelly, and then breaking down into a white powder.

(2.) In distillation of the chalk-stone the lithic acid is decomposed, and yields the usual products of animal substances, *viz.* a fetid alkaline liquor, volatile alkali, and a heavy fetid oil, leaving a spongy coal; which when burnt in open air fuses into a white salt, that does not deliquesce, but dissolves

entirely in water, is alkaline, and when saturated with nitrous acid gives rhomboidal crystals.

These characteristic properties prove it to be mineral alkali.

(3.) Caustic vegetable alkali poured upon the chalk-stone, and warmed, dissolves the whole without emitting any smell of volatile alkali. From which it appears, that the volatile alkali obtained by distillation is a product arising from a new arrangement of elements, not so combined in the substance itself.

(4.) Water aided by a boiling heat dissolves a very small proportion of the gouty concretion, and retains it when cold. The lithic acid thus dissolved in combination with the alkali, is rather more than would be dissolved alone; so that by addition of marine acid it may be separated. While the solution continues warm no precipitate is formed; but as it cools, the lithic acid crystallizes on the sides of the vessel, in the same manner as the crystals called red sand do, when an acid is added to recent urine.

The gouty concrete may be easily formed by uniting the ingredients of which I have found it to consist.

(5.) If a fragment of lithic acid be triturated with some mineral alkali and a little warm water, they unite, and after the superfluous alkali has been washed out, the remainder has every chemical property of gouty matter.

The acid will not sublime from it, but is decomposed (2.) by heat: the alkali may be extracted by the vitriolic or marine (1.) or indeed by most acids. The compound requires a large quantity of water for its solution (4.), and while warm the solution yields no precipitate by the addition of an acid; but upon its

cooling the lithic crystals form, as in the preceding experiment.

In each case the crystals are too small for accurate examination, but I have observed, that by mixing a few drops of caustic vegetable alkali to the solution previous to the decomposition, they may be rendered somewhat larger. At the first precipitation, the crystals from gouty matter were not similar to those of lithic acid; but by redissolving the precipitate in water with the addition of a little caustic vegetable alkali, and decomposing the solution as before, while hot, the crystals obtained were perfectly similar to those of lithic acid procured by the same means.

Such then are the essential ingredients of the gouty concretion. But there might probably be discovered, by an examination of larger masses than I possess, some portion of common animal fibre or fluids intermixed; but whatever particles of heterogeneous matter may be detected, they are in far too small proportion to invalidate the general result, that 'gouty matter 'is lithiated soda.'

The knowledge of this compound may lead to a further trial of the alkalies which have been observed by Dr. CULLEN to be apparently efficacious in preventing the returns of this disease (First Lines, DLVIII.); and may induce us, when correcting the acidity to which gouty persons are frequently subject, to employ the fixed alkalies, which are either of them capable of dissolving gouty matter, in preference to the earths (termed absorbent) which can have no such beneficial effect.

Fusible Calculus.

My next subject of inquiry has been a species of calculus, that was first ascertained to differ from that of SCHEELE by Mr. TENNANT; who found that when urged by the heat of a blow-pipe, instead of being nearly consumed, it left a large proportion fused into an opaque white glass, which he conjectured to be phosphorated lime united with other phosphoric salts of the urine, but never attempted a more minute analysis.

Stones of this kind are always whiter than those described by SCHEELE, and some specimens are perfectly white. The greater part of them have an appearance of sparkling crystals, which are most discernible where two crusts of a laminated stone have been separated from each other.

I lately had an opportunity of procuring these crystals alone, voided in the form of a white sand, and thence of determining the nature of the compound stone, in which these are cemented by other ingredients.

The crystals consist of phosphoric acid, magnesia, and volatile alkali: the stone contains also phosphorated lime, and generally some lithic acid.

The form of the crystals is a short trilateral prism, having one angle a right angle, and the other two equal, terminated by a pyramid of three or six sides.

(6.) By heat the volatile alkali may be driven off from the crystals, and they are rendered opaque (or may be partially fused). The phosphorated magnesia may then be dissolved in nitrous acid; and by addition of quicksilver dissolved in the same acid, a precipitate of phosporated quicksilver is obtained,

from which the quicksilver may be expelled by heat, and the acid procured separate. By addition of vitriolic acid to the remaining solution, Epsom salt is formed, and may be crystallized, after the requisite evaporation of the nitrous acid, and separation of any redundant quicksilver.

(7.) These crystals require a very large quantity of water for their solution, but are readily soluble in most if not all acids; *viz.* vitriolic, nitrous, marine, phosphoric, saccharine, and acetous; and when precipitated from them re-assume the crystalline form.

(8.) From the solution in marine acid, sal ammoniac may be obtained by sublimation.

(9.) Although the analysis is satisfactory, the synthetic proof is (if possible) still more so. After dissolving magnesia in phosphoric acid, the addition of volatile alkali immediately forms the crystalline precipitate, having the same figure and properties as the original crystals.

(10.) If volatile alkali be cautiously mixed with recent urine, the same compound will be formed; the first appearance that takes place when a sufficient quantity of alkali has been gradually added, is a precipitate of these triple crystals.

These constitute the greater part of the fusible stone; so that a previous acquaintance with their properties is necessary, in order to comprehend justly the nature of the compound stone in which they are contained.

The most direct analysis of the compound stone is effected by the successive action of distilled vinegar, marine acid, and caustic vegetable alkali.

(11.) Distilled vinegar acts but slowly upon the calculus when entire; but when powdered, it immediately dissolves the

triple crystals, which may be again precipitated from it as crystals by volatile alkali; and if the solution has not been aided by heat, scarcely any of the phosphorated lime will be found blended with them.

In one trial the triple crystals exceeded $\frac{1}{10}$ of the quantity employed; but it seemed unnecessary to determine the exact proportion which they bear to the other ingredients in any one instance, as that proportion must vary in different specimens of such an assemblage of substances not chemically combined.

Marine acid, poured on the remainder, dissolves the phosphorated lime, leaving a very small residuum.

This is soluble in caustic vegetable alkali entirely, and has every other property of mere lithic acid.

The presence of volatile alkali in the compound stone may be shewn in various ways.

(12.) In the distillation of this stone there arises, first volatile alkali in great abundance, a little fetid oil, and lithic acid. There remains a large proportion charred. Water poured upon the remaining coal dissolves an extremely small quantity of a salt, apparently common salt, but too minute for accurate examination. Distilled vinegar dissolves no part of it even when powdered. Marine acid dissolves the phosphorated lime and phosphorated magnesia, leaving nothing but a little charcoal. From this solution vitriolic acid occasions a precipitate of selenite, after which triple crystals may be formed by ad ition of volatile alkali.

(13.) Marine acid also acts readily upon a fragment of the stone, leaving only yellowish laminæ of lithic acid. When the solution has been evaporated to dryness, sal ammoniac may be sublimed from it; and the two phosphorated earths are found

combined with more or less of marine acid, according to the degree of heat applied. If the proportion of the earth is wished to be ascertained, acid of sugar will separate them most effectually, by dissolving the phosphorated magnesia, and forming an insoluble compound with the lime.

(14.) Caustic vegetable alkali has but little effect upon the entire stone; but if heated upon the stone in powder, a strong effervescence takes place from the escape of alkaline air, and the menstruum is found to contain lithic acid precipitable by any other acid. Some phosphoric acid also, from a partial decomposition of the triple crystals, is detected by nitrated quicksilver.

(15.) The triple crystals alone are scarcely fusible under the blow-pipe; phosphorated lime proves still more refractory; but mixtures of the two are extremely fusible, which explains the fusibility of the calculus.

The appearance of the lithic strata, and the small proportion they bear to the other ingredients, shews that they are not an essential part, but an accidental deposit, that would be formed on any extraneous substance in the bladder, and which probably in this instance concretes during any temporary interval that may occur in the formation of the crystals.

I come now to what has been called

Mulberry Calculus.

This stone, though by no means overlooked, and though pointed out as differing from other species, has not, to my knowledge, been subjected to any farther analysis than is given, in the Second Volume of the Medical Transactions, by Dr. Dawson, who found that his lixivium had little or no effect

upon it; and in the Phil. Trans. by Mr. LANE, who, among other simple and compound stones, gives an account of the comparative effects of lixivium and heat upon a few specimens of mulberry calculus (*viz.* No. 7, 8, 9, 10.); but neither of these writers attempted to ascertain the constituent parts.

Though the name has been confined to such stones as, from their irregularly knotted surface and dark colour, bear a distant resemblance to that fruit, I find the species, chemically considered, to be more extensive, comprehending also some of the smoothest stones we meet with; of which one in my possession is of a much lighter colour, so as to resemble in hue, as well as smoothness, the surface of a hemp-seed. From this circumstance it seems not improbable, that the darkness of irregular stones may have arisen from blood voided in consequence of their roughness.

The smooth calculus I find to consist of lime united with the acids of sugar and of phosphorus. The rougher specimens have generally some lithic acid in their interstices.

(16.) Caustic vegetable alkali acquires a slight tinge from a fragment of this kind of stone, but will not dissolve it. When powdered it is thereby purified from any quantity of lithic acid that it may contain. Phosphoric acid will then dissolve out the phosphorated lime, and the remainder, after being washed, may be decomposed by the vitriolic. The affinity of this acid for a certain proportion of lime is superior even to that of acid of sugar; selenite is formed, and the acid of sugar may be crystallized, and by the form of its crystals recognized, as well as by every other property. It is easily soluble, occasions a precipitate from lime water, and from a solution of selenite,

and with mineral alkali forms a salt that requires a large quantity of water for its solution.

(17.) When the stone has been finely powdered, marine acid will slowly dissolve all but any small quantity of lithic matter which it may contain. After the solution has been evaporated to dryness no part is then soluble in water, the marine acid being wholly expelled. When the dried mass is distilled with a greater heat, the saccharine acid is decomposed, and a sublimate formed, still acid and still crystallizable, but much less soluble in water, and which does not precipitate lime from lime water. After distillation the remainder contains phosphorated lime, pure lime, and charcoal; and when calcined in the open air, the charcoal is consumed and the whole reduced to a white powder. The two former may be dissolved in marine acid, which when evaporated to dryness will be retained only by the lime; so that water will then separate the muriated lime, and the phosphorated may afterwards be submitted to the usual analysis.

Bone-earth Calculus.

Beside that of SCHEELE, and the two already noticed, there is also a fourth species of calculus, occasionally formed in the bladder, distinct in its appearance, and differing in its component parts from the rest; for it consists entirely of phosphorated lime.

Its surface is generally of a pale brown, and so smooth as to appear polished; when sawed through, it is found very regularly laminated; and the laminæ in general adhere so slightly to each other, as to separate with ease into concentric crusts. In a specimen with which I was favoured by Dr. BAILLIE, each

lamina is striated in a direction perpendicular to the surface, as from an assemblage of crystallized fibres.

This calculus dissolves entirely, though slowly, in marine or nitrous acid, and, consisting of the same elements as earth of bones, may undergo a similar analysis, which it cannot be necessary to particularize.

By the blow-pipe it is immediately discovered to differ from other urinary calculi: it is at first slightly charred, but soon becomes perfectly white, still retaining its form, till urged with the utmost heat from a common blow-pipe, when it may at length be completely fused. But even this degree of fusibility is superior to that of bones. The difference consists in an excess of calcareous earth contained in bones, which renders them less fusible. This redundant portion of lime in bones renders them also more readily soluble in marine acid, and may, by evaporation of such a solution, be separated, as in the last experiment upon mulberry calculus. The remaining phosphorated lime may be re-dissolved by a fresh addition of marine acid; and being now freed from redundant lime, will, upon evaporation of the marine acid, assume a crystalline form. As the laminated calculus contains no excess of lime, *that* will at once yield such crystals: their appearance will be described in the succeeding experiment.

Calculus from the Prostate Gland.

There is still another calculus of the urinary passages, though not of the bladder itself, which deserves notice, not from the frequency of its occurrence, but from having been supposed to give rise to stone in the bladder. I mean the small stones which are occasionally found in the prostate gland. Those

that I have seen, and which, by favour of Mr. ABERNETHY, I have had an opportunity of examining, were from the size of the smallest pin's head to that of pearl barley, in colour and transparency like amber, and appeared originally to have been spherical; but from contiguity with others, some had flattened surfaces, so as at first sight to appear crystallized.

These I find to be phosphorated lime in the state of neutralization, tinged with the secretion of the prostate gland.

(18.) A small fragment being put into a drop of marine acid, on a piece of glass over a candle, was soon dissolved; and upon evaporation of the acid, crystallized in needles, making angles of about 60° and 120° with each other.

Water dropped on the crystals would dissolve no part of them; but in marine acid they would re-dissolve, and might be re-crystallized.

(19.) Vitriolic acid forms selenite with the calcareous earth.

(20.) By acid of nitrated quicksilver, phosphoric acid is readily obtained.

(21.) When heated this calculus decrepitates strongly; it next emits the usual smell of burnt animal substances, and is charred, but will not become white though partially fused. It still is soluble in marine acid, and will in that state crystallize more perfectly than before. Hence I conclude, that these stones are tinged with the liquor of the prostate gland, which in their original state (18.) somewhat impedes the crystallization.

This crystallization from marine acid is so delicate a test of the neutral phosphorated lime, that I have been enabled by that means to detect the formation of it, although the quantities were very minute. The particles of sand which are so

generally to be felt in the pineal gland, have this for their basis; for I find that after calcination they crystallize perfectly from marine acid.

I have likewise met with the same compound in a very pure state, and soft, contained in a cyst under the pleura costalis.

On the contrary, ossifications (properly so called) of arteries and of the valves of the heart, are similar to earth of bones, in containing the redundant calcareous earth; and I believe also those of veins, of the bronchiæ, and of the tendinous portion of the diaphragm, have the same excess; but my experiments on these were made too long since for me to speak with certainty.

To these I may also add the incrustation frequently formed upon the teeth, which, in the only two specimens that I have examined, proved to be a similar compound, with a very small excess of lime.

Though I do not at present presume to draw conclusions with regard to the treatment of all the diseases in question, some inferences cannot pass unobserved.

The sand from the pineal gland, from its frequency hardly to be called a disease, or when amounting to disease most certainly not known by its symptoms, would, at the same time, if known, be wholly out of the reach of any remedy.

The calculi of the prostate are too rare, perhaps, to have been ever yet suspected in the living body, and are but indirectly worthy of notice. For if by chance one of them should be voided with the urine, a knowledge of its source would guard us against an error we might otherwise fall into, of proposing the usual solvents for urinary calculi.

The bone-earth calculus, although so nearly allied to the

last, is still manifestly different, and cannot be supposed to originate from that source; but if ever the drinking of water impregnated with calcareous earth gave rise to a stone in the bladder, this would most probably be the kind generated, and the remedy must evidently be of an acid nature.

With respect to the mulberry calculus, I fear that an intimate knowledge of its properties will leave but small prospect of relief from any solvent; but by tracing the source of the disease we may entertain some hopes of preventing it. As the saccharine acid is known to be a natural product of a species of oxalis, it seems more probable that it is contained in some other vegetables or their fruits taken as aliment, than produced by the digestive powers, or secreted by any diseased action of the kidneys. The nutriment would therefore become a subject of minute inquiry, rather than any supposed defect of assimilation or secretion.

When a calculus is discovered, by the evacuations, to be of the fusible kind, we seem to be allowed a more favourable prospect in our attempts to relieve: for here any acid that is carried to the bladder will act upon the triple crystals, and most acids will also dissolve the phosphorated lime; while alkalies, on the contrary, would rather have a tendency to add to the disease.

Although, from want of sufficient attention to the varieties of sediment from urine, and want of information with regard to the diversity of urinary calculi, the deposits peculiar to each concretion are yet unknown; it seems probable that no long course of observation would be necessary to ascertain with what species any individual may be afflicted.

The lithic, which is by far the most prevalent, fortunately

affords us great variety of proofs of its presence. Particles of red sand (as they are called) are its crystals. Fragments also of larger masses, and small stones, are frequently passed; and it is probable that the majority of appearances in the urine called purulent, are either the acid itself precipitated too quickly to crystallize, or a neutral compound of that acid with one of the fixed alkalies.

Beside this species, the fusible calculus has afforded decisive marks of its presence in the case which furnished me with my specimen of triple crystals; and by the description given by Mr. FORBES (in his Treatise upon Gravel and Gout, ed. 1793, p. 65.) of a white crystallized precipitate, I entertain no doubt that his patient laboured under that variety of the disease.

XVIII. *Experiments on carbonated hydrogenous Gas; with a View to determine whether Carbon be a simple or a compound Substance.* *By Mr.* William Henry. *Communicated by Mr.* Thomas Henry, *F. R. S.*

Read June 29, 1797.

The progress of chemical science depends not only on the acquisition of new facts, but on the accurate establishment, and just valuation, of those we already possess: for its general principles will otherwise be liable to frequent subversions; and the mutability of its doctrines will but ill accord with the unvaried order of nature. Impressed with this conviction, I have been induced to examine a late attempt to withdraw from its rank among the elementary bodies, one of the most interesting objects of chemistry. The inferences respecting the composition of charcoal, deduced by Dr. Austin from his experiments on the heavy inflammable air,* lead to changes so numerous in our explanations of natural phænomena, that they ought not to be admitted without the strictest scrutiny of the reasoning of this philosopher, and an attentive repetition of the experiments themselves. In the former, sources of fallacy may, I think, be easily detected; and in the latter, there is reason to suspect that Dr. Austin has been misled by inattention to some collateral circumstances. Several chemists, however, of distinguished rank have expressed themselves satisfied with the

* Phil. Trans. Vol. LXXX. p. 51.

evidence thus produced in favour of the composition of charcoal; and amongst these it may be sufficient to mention Dr. BEDDOES, who has availed himself of the theory of Dr. AUSTIN, in explaining some appearances that attend the conversion of cast into malleable iron.*

The heavy inflammable air, having been proved to consist of a solution of pure charcoal in light inflammable air, is termed, in the new nomenclature, carbonated hydrogenous gas. By repeatedly passing the electric shock through a small quantity of this gas, confined in a bent tube over mercury, Dr. AUSTIN found that it was permanently dilated to more than twice its original volume. An expansion so remarkable could not, as he observes, be occasioned by any other known cause than the evolution of light inflammable air.

When the electrified air was fired with oxygenous gas, it was found that more oxygen was required for its saturation than before the action of the electric fluid; which proves that, by this process, an actual addition was made of combustible matter.

The light inflammable air disengaged by the electrization, proceeded, without doubt, from the decomposition of some substance within the influence of the electric fluid, and not merely from the expansion of that contained in the carbonated hydrogenous gas: for had the quantity of hydrogen remained unaltered, and its state of dilatation only been changed, there would not, after electrization, have been any increased consumption of oxygen.

The only substances in contact with the glass tube and mercury, in these experiments, besides the hydrogen of the dense

* Phil. Trans. Vol. LXXXI.

inflammable gas, were carbon and water; which last, though probably not a constituent of gases, is, however, copiously diffused through them. If the evolved hydrogen proceeded from the decomposition of the former of these two substances, it is evident that a certain volume of the carbonated hydrogenous gas must yield, after electrization, on combustion with oxygen, less carbonic acid than an equal volume of non-electrified gas; or, in other words, the inflammation of 20 measures of carbonated hydrogen, expanded by electricity from 10, should not afford so much carbonic acid as 10 measures of the unelectrified.

From the fact which has been before stated, respecting the increased consumption of oxygen by the electrified air, it follows, that in determining the quantity of its carbon by combustion, such an addition of oxygen should be made, to that necessary for the saturation of the gas before exposure to the electric shock, as will completely saturate the evolved hydrogen. For if this caution be not observed, we may reasonably suspect that the product of carbonic acid is diminished, only because a part of the heavy inflammable air has escaped combustion. It might, indeed, be supposed, that in consequence of the superior affinity of carbon for oxygen, the whole of the former substance, contained in the dense inflammable gas, would be saturated, and changed into carbonic acid, before the attraction of hydrogen for oxygen could operate in the production of water. But I have found that the residue, after inflaming the carbonated hydrogenous gas with a deficiency of oxygen, and removing the carbonic acid, is not simply hydrogenous but carbonated hydrogenous gas.

In the 2d, 5th, and 6th of Dr. Austin's experiments, in which the quantity of carbon, in the electrified gas, was exa-

mined by deflagrating it with oxygen, the combustion was incomplete, because a sufficiency of oxygen was not employed; and Dr. AUSTIN himself was aware that, in each of them, "a "small quantity of heavy inflammable air might escape unal-"tered." It is observable, also, that the product of carbonic acid, from the electrified gas, increased in proportion as the combustion was more perfect. We may infer, therefore, that if it had been complete, there would have been no deficiency of this acid gas, and consequently no indication of a decomposition of charcoal. A strong objection, however, is applicable to these, as well as to most of Dr. AUSTIN's experiments, that the residues were not examined with sufficient attention. In one instance we are told, that the remaining gas was inflammable, and in another, that it supported combustion like vital air. I need hardly remark, that a satisfactory analysis cannot be attained of any substance, without the most scrupulous regard not only to the qualities, but to the precise quantities of the products of our operations.

To the 8th and 9th experiments, the objection may be urged with additional weight, which has been brought against the preceding ones, that the quantity of oxygen, instead of being duly increased in the combustion of the electrified gas, was, on the contrary, diminished. Thus, in the 8th experiment, 2,83 measures of carbonated hydrogen were inflamed with 4,58 measures of oxygenous gas; but in the 9th, though the 2,83 measures were dilated to 5,16, and had therefore received a considerable addition of combustible matter, the oxygen employed was only 4,09. To the rest of Dr. AUSTIN's experiments either one or both of the above objections are applicable.

The first and most important step, therefore, in the repetition

of these experiments, is to determine, whether the carbonated hydrogenous gas really sustains, by the process of electrization, a diminution of its quantity of carbon; because, should this be decided in the negative, we derive from the fact a very useful direction in ascertaining the true source of the evolved hydrogen. The following experiments were therefore made with a view to decide this question, and the error of Dr. AUSTIN, in employing too little oxygen, was carefully avoided.*

Experiment 1. In a bent tube, standing inverted over mercury, 94,5 measures of carbonated hydrogenous gas from acetite of pot-ash, were mixed with 107,5 of oxygen. The total, 202, was reduced by an explosion to 128,5, and was farther contracted by lime water to 54. A solution of hepar sulphuris left only 23 measures.

The diminution by lime water, *viz.* 74,5 measures, makes known to us the quantity of carbonic acid afforded by the combustion of 94,5 measures of carbonated hydrogenous gas: and the residue after the action of hepar sulphuris, *viz.* 23 measures, gives the proportion of azotic gas contained in the carbonated hydrogen; for the oxygenous gaz employed, which was procured

* The apparatus employed in these experiments, was the ingenious contrivance of Mr. CAVENDISH, and is described in the LXXV. Vol. of the Philosophical Transactions. In dilating the gas, I sometimes used a straight tube, furnished with a conductor, in the manner of Dr. PRIESTLEY, (see his Experiments on Air, Vol. I. plate I. fig. 16.). The bulk of the gases introduced, and their volume after the various experiments, were ascertained by a moveable scale, and by afterwards weighing the mercury which filled the tube to the marks on the scale; by which means I was spared the trouble of graduating the syphons. Each grain of mercury indicates one measure of gas; and though the smallness of the quantities submitted to experiment may be objected to, yet this advantage was gained, that the electrified gas could be fired at one explosion, as was done in the 4th, 6th, and 8th experiments. Errors, from variations of temperature and atmospherical pressure, were carefully avoided.

from oxygenated muriate of pot-ash, was so pure, that the small quantity used in this experiment could not contain a measurable portion of azotic gas.

Experiment 2. The same quantity of carbonated hydrogen was expanded by repeated electrical shocks to 188 measures. The addition of hydrogenous gas, therefore, amounted to 98,5. The gas, thus dilated, was fired, at different times, with 892,5 measures of oxygenous gas; and the residue, after these several explosions, was 203 measures. Lime water reduced it to 128,5, and sulphure of pot-ash to 19,5. In this instance, as in the former one, the product of carbonic acid is 74,5 measures.

Finding, from the first experiment and other similar ones, that the carbonated hydrogenous gas, which was the subject of them, contained a very large admixture of azotic gas, I again submitted to distillation a quantity of the acetite of pot-ash, with every precaution to prevent the adulteration of the product with atmospherical air. Such an adulteration, I have observed, impedes considerably the dilatation of the gas, and for a time even entirely prevents it. This explains the failure, which some experienced chemists have met with, in their attempts to expand the carbonated hydrogenous gas by electricity. Gas which is thus vitiated becomes, however, capable of expansion, after exposure to the sulphure of pot-ash.

Experiment 3. Carbonated hydrogen 340 measures were exploded with the proper proportion of oxygenous gas. The carbonic acid produced amounted to 380 measures, and the residue of azotic gas was 20 measures.

Experiment 4. The same quantity, when expanded to 690, gave on combustion 380 measures of carbonic acid, and 19,8 of azotic gas.

Experiment 5. Three hundred and fifteen measures of carbonated hydrogen yielded 359 measures of carbonic acid, and 18,5 measures of azote.

Experiment 6. The same quantity, after expansion to 600, afforded the same products of carbonic acid and azotic gases.

Experiments 7 and 8. As much carbonic acid was obtained by the combustion of 408 measures of carbonated hydrogenous gas, expanded from 200, as from 200 measures of the non-electric fired gas; and the residues of azotic gas were the same in both cases.

It is unnecessary to state the particulars of several other experiments, similar to those above related, which were attended with the same results. They sufficiently prove that the action of the electric spark, when passed through carbonated hydrogenous gas, is not exerted in the decomposition of carbon: for the same quantity of this substance is found after as before electrization. Even granting that charcoal is a compound, the constituents of which are held together by a very forcible affinity, it does not appear likely that the agency of the electric shock, which seems, in this instance, analogous to that of caloric, should effect its decomposition under the circumstances of these experiments. For it is a known property of charcoal to decompose water, when aided by a high temperature; and its union with oxygen is a much more probable event, when this body is present, than a separation into its constituent principles. As an argument, also, that water is the source of the light inflammable air in this process, it may be observed, that the dilatation in Dr. AUSTIN's experiments could never be carried much farther than twice the

original bulk of the gas.* This fact evidently implies that the expansion ceased only in consequence of the entire destruction of the matter, whose decomposition afforded the light inflammable air, and this substance could not be carbon, because Dr. AUSTIN admits that a large portion, and I have shewn that the whole of it, still remains unaltered.

If the dilatation of the carbonated hydrogenous gas arose from the decomposition of water, the effect should cease when this fluid is previously abstracted. To ascertain whether this consequence would really follow, I exposed a portion of the gas, for several days before electrization, to dry caustic alkali. On attempting its expansion, I found that it could not be carried beyond one-sixth the original bulk of the gas. By 160 very strong explosions it attained this small degree of dilatation, but 80 more produced not the least effect; though the former number would have been amply sufficient to have dilated the gas, in its ordinary state, to more than twice its original volume. A drop or two of water being admitted to this portion of gas, the expansion went on as usual; and I may here observe, that when a little water gained admission into the tube along with the gas, in any experiment, which often happened before I had acquired sufficient expertness in transferring the air from water to mercury, the dilatation went on with remarkable rapidity.

* "After the inflammable air has been expanded to about double its original bulk," says Dr. AUSTIN, "I do not find that it increases further by continuing the shocks. "Conceiving that the progress of the decomposition was impeded by the mixture of the "other airs with the heavy inflammable, I passed the spark through a mixture of the "heavy inflammable air and light inflammable; but the expansion succeeded nearly as "well as when the heavy inflammable was electrified alone." Phil. Trans. Vol. LXXX. p. 52.

Carbonic acid gas, according to the discovery of M. MONGE,* undergoes, when submitted to the electric shock, a change similar to that effected on the carbonated hydrogen; and the expansion has been shewn, by Messrs. LANDRIANI and VAN MARUM,† to be owing to the same cause, *viz.* the extrication of light inflammable air. The added gas, M. MONGE ably contends, cannot proceed from any other source than the water held in solution by all aeriform bodies, the oxygen of which he supposes to combine with the mercury. That the decomponent of the water, however, in the experiments which I have described, is not a metallic body, will appear highly probable when we reflect that there is present in them a combustible substance, *viz.* charcoal, which attracts oxygen much more strongly than metals; and the following experiments evince that the mercury, by which the air was confined, had no share in producing the phænomena.

Experiment 9. A portion of carbonated hydrogenous gas was introduced into a glass tube closed at one end, into which a piece of gold wire was inserted that projected both within and without the cavity of the tube. The open end of the tube was then closed by a stopper perforated also with gold wire, so that electric shocks could be passed through the confined air, without the contact of any metal that has the power of decomposing water. On opening the tube with its mouth downwards, under water, a quantity of air immediately rushed out.

Experiment 10. The dilatation of the gas was found to proceed very rapidly when standing over water, and exposed to the action of the electric fluid, conveyed by gold conductors.

We have only, therefore, in the two preceding experiments,

* 29 *Journal de Physique*, 277. † 2 *Annales de Chimie*, 273.

one substance in contact with the gas which is capable of decomposing water, *viz.* charcoal. The union of this body with the oxygen of the water would be rendered palpable by the formation of carbonic acid; but Dr. AUSTIN did not observe that any precipitation was occasioned in lime water, by agitating it with the electrified gas. On passing up syrup of violets to the electrified air, with the expectation of its indicating the volatile alkali, as in the experiments of Dr. AUSTIN, no change of colour took place, though the test was of unexceptionable purity. On examining, however, whether any alteration of bulk had been produced in the air by the contact of this liquid, it appeared that of 709 measures, 100 had been absorbed. Suspecting that the absorption was owing to the presence of carbonic acid, I introduced some lime water to a volume of the expanded gas amounting to 556 measures, when they were immediately reduced to 512. The contraction would probably have been still more remarkable if the gas had been farther expanded before the admission of the liquid. The change in the lime water was very trifling; but my friend Mr. RUPP, who witnessed this as well as several of the other experiments, and who is much conversant in the observation of chemical facts, was satisfied that, after a while, he saw small flocculi of a precipitate on the surface of the mercury. This contraction of bulk cannot be ascribed to any other cause than the absorption of carbonic acid; for besides the fact that the colour of syrup of violets and of turmeric, which I also tried, were not affected by exposure to the electrified gas, I have this objection to the absorbed gas being ammoniac, that no diminution either of bulk or transparency occurred on the admixture of muriatic acid gas with the electrified air; whereas ammoniac would

have been exhibited under the form of a neutral salt. When water was passed up to this mixture of the two gases, there was an absorption not only of the muriatic gas, but of something more.

Conceiving that the demolition of charcoal, by the action of the electric fluid, was sufficiently proved by his experiments, Dr. AUSTIN assigns the evolved hydrogen as one of its constituents, and the other he concludes to be azote. This inference, however, rests almost entirely upon estimates, in which material errors may be discovered. Some of these it may be well to point out, for the satisfaction of such as have acquiesced in Dr. AUSTIN's opinion.

The carbonated hydrogenous gas submitted to Dr. AUSTIN's experiments clearly appears, from his own account, to have been largely adulterated with azotic gas. One source of its impurity he has disclosed, by informing us that the gas "had "been very long exposed to water;"* for Dr. HIGGINS has somewhere shewn that the heavy inflammable air, after standing long over water, leaves a larger residue of azote, on combustion, than when recently prepared.† It is probable also, that the proportion of azote derived from the water, would increase with the time of its exposure; and thus a fertile source of error is suggested, which appears wholly to have escaped Dr. AUSTIN's attention. In repeating his experiments, I was careful that comparative ones, on two equal quantities of the

* 80 Phil. Trans. 54.

† Similar facts respecting the deterioration of other gases, by standing over water, may be seen in Dr. PRIESTLEY's Experiments on Air, Vol. I. p. 59, 158. I found that oxygenous gas, from oxygenated muriate of pot-ash, acquired, by exposure a few weeks to water, .125 its bulk of azotic gas.

electrified and unelectrified gas, should be made without the intervention of any time that could vary the proportion of azote in either of the gases.

To the 9th experiment, in which the quantity of azote seems to have been increased by electrization, I must repeat the objection, that a sufficiency of oxygenous gas was not used in the combustion. In the 8th experiment, 2,83 of the unelectrified air were fired with 4,17 oxygenous gas, and only 0,15 of the latter remained above what was sufficient for saturation; but in the 9th, though the 2,83 measures were expanded to 5,16, the quantity of oxygen employed was 0,08 less than in the former experiment; and it may therefore be presumed that a small quantity of inflammable air might escape unaltered, and might add apparently to the product of azote. In the 8th experiment also, the portion of oxygenous gas that was more than sufficient to saturate the carbonated hydrogen, would probably combine, in part, with the remaining azote, as in the experiments of Dr. HIGGINS* and Dr. PRIESTLEY.† But in the 9th, the quantity of oxygenous gas was hardly sufficient to saturate both kinds of inflammable air after electrization, and could not therefore diminish the azotic gas. When the proportion of oxygen is duly increased, and the inflammation of the electrified air is performed in small portions, there is no augmentation, but on the contrary a decrease of the quantity of the azote, as will appear on comparing the 1st and 2d of the experiments which I have related.

Two circumstances were observed, in the experiments of Dr. AUSTIN, which have not been noticed in the preceding account

* Experiments and Observations on acetous Acid, *&c.* p. 295.

† 79 Phil. Trans. 7.

of the repetition of them, *viz.* the appearance of a deposit from the carbonated hydrogenous gas during its electrization, and the formation of ammoniac by the same process. In some experiments, which I made on the first portion of gas, both these facts were sufficiently apparent; but neither of them occurred on electrifying the gas which was afterwards procured. Suspecting that the cessation of them arose from the superior purity of the latter portion from azotic gas, I passed the electric shock through a mixture of carbonated hydrogen with about one-fourth its bulk of azote, and thus again produced the precipitate, which would have been of a white colour, if it had not been obscured by minute globules of mercury, that were driven upwards by the force of the explosion. An infusion of violets was tinged green when admitted to the electrified gas; but the change of colour did not occur instantly, as happens from the absorption of ammoniacal gas; and required for its production that the liquid should be brought extensively into contact with the inner surface of the tube. From this effect on a blue vegetable colour, we may infer that the precipitate was an alkaline substance, and probably the carbonate of ammoniac; but the quantity was much too minute to be the subject of more decisive experiment.

I shall conclude this memoir, with a brief summary of the facts that are established by the preceding experiments.* Those included under the first head are deducible from the experiments of Dr. AUSTIN.

* Since this paper was written I have extended the inquiry to phosphorated hydrogenous gas, which expands equally with the carbonated hydrogen; loses its property of inflaming when brought into contact with oxygenous gas; and affords evident traces of a production of phosphorous or phosphoric acid.

1. Carbonated hydrogenous gas, in its ordinary state, is permanently dilated by the electric shock to more than twice its original volume; and as light inflammable air is the only substance we are acquainted with, that is capable of occasioning so great an expansion, and of exhibiting the phænomena that appear on firing the electrified gas with oxygen, we may ascribe the dilatation to the production of hydrogenous gas.

2. The hydrogenous gas evolved by this process does not arise from the decomposition of charcoal; because the same quantity of that substance is contained in the gas after, as before electrization.

3. The hydrogenous gas proceeds from decomposed water; because when this fluid is abstracted as far as possible from the carbonated hydrogenous gas, before submitting it to the action of electricity, the dilatation cannot be extended beyond one-sixth its usual amount.

4. The decomponent of the water is not a metallic substance, because carbonated hydrogenous gas is expanded when in contact only with a glass tube and gold, a metal which has no power of separating water into its formative principles.

5. The oxygen of the water (when the electric fluid is passed through carbonated hydrogenous gas, that holds this substance in solution), combines with the carbon, and forms carbonic acid. This production of carbonic acid, therefore, adds to the dilatation occasioned by the evolution of hydrogenous gas.

6. There is not, by the action of the electric matter on carbonated hydrogenous gas, any generation of azotic gas.

7. Carbon, it appears, therefore, from the united evidence of these facts, is still to be considered as an elementary body; that is, as a body with the composition of which we are unacquainted, but which may nevertheless yield to the labours of some future and more successful analyst.

XIX. *Observations and Experiments on the Colour of Blood.* By William Charles Wells, *M.D. F.R.S.*

Read July 6, 1797.

Dr. Priestley is, I believe, the only person who has hitherto attempted to shew by what means common air brightens the colour of blood, which has been for some time exposed to it.* His opinion is, that the air produces this effect by depriving the blood of its phlogiston; for blood, according to the same author, is wonderfully fitted both to imbibe and to part with phlogiston, becoming black when charged with that principle, but highly florid when freed from it. Various arguments may be brought to prove that this opinion is erroneous, even upon the admission of such a principle of bodies as phlogiston. It may be said, for instance, that it is contrary to the laws of chemical affinity, that the same mass should, at one time, convert pure into phlogisticated air, by giving out its phlogiston, and immediately after reconvert phlogisticated into pure air, by imbibing that principle; both which changes are supposed by Dr. Priestley to be induced by blood upon those airs. Again; it may be urged, that, since the neutral salts, and the different alkalis, when saturated with fixed air, produce the same effect as common air upon the colour of blood, if common air acts by attracting phlogiston, those other bodies must have a similar operation. But surely it cannot be

* Phil. Trans. for 1776.

thought, that the mild volatile alkali, which has been supposed by chemists to superabound with phlogiston, can yet attract it from blood. It appears to me, however, unnecessary to bring any further arguments of this kind against the opinion of Dr. PRIESTLEY, since the following experiments will, I expect, be thought sufficient to shew, in opposition to what is taken for granted by him in the whole of his inquiry, that the alteration induced upon the colour of blood, both by common air and the neutral salts, is altogether independent of any change effected by them upon its colouring matter.

I infused a piece of black crassamentum of blood in distilled water, and immediately after covered the containing vessel closely, to prevent the access of air. Having obtained by this means a transparent solution of the red matter of blood, nearly free from serum and coagulable lymph, I exposed a quantity of it to the open air, in a shallow vessel, and poured an equal quantity into a small phial, which was then well closed. When the first portion of the solution had been exposed to the air for several hours, I decanted it into a phial, of the same size and shape as that which contained the second portion, and having added to it as much distilled water as was sufficient to compensate the loss it had suffered by evaporation, I now compared the two together, and found them to be exactly of the same colour, with regard both to kind and degree. I afterwards poured two other equal quantities of the red solution into two phials of the same size and shape. To one I added a little of a solution of nitre in water, and to the other as much distilled water. Upon comparing the two mixtures together, I found that they also possessed precisely the same colour. Lastly, I cut a quantity of dark crassamentum

of blood into thin slices, and exposed them to common air. When they became florid, I put them into a phial containing distilled water. I then took as much of the same crassamentum, which was still black, and infused it in an equal quantity of distilled water, contained in a phial similar in size and shape to the former. The two solutions which were thus obtained, one from florid blood, the other from black blood, were, notwithstanding, of precisely the same colour. These experiments were frequently repeated, and were attended with the same results, as often as I used certain precautions, which shall be mentioned hereafter, as the reasons for them will then be more readily understood than they can be at present.

Assuming therefore as proved, that neither common air, nor the neutral salts (for all those I have tried are similar to nitre in this respect) change the colour of the red matter of blood; I shall now attempt to explain the manner in which those substances give, notwithstanding, to black blood a florid appearance; premising, however, some observations upon the colours of bodies in general.

It was the opinion of KEPLER,* that light is reflected without colour from the surfaces of bodies; which he says is easily proved, by exposing to the sun's light a number of cups filled with transparent liquors of different colours, and receiving the reflexions from them upon a white ground in a dark place. ZUCCHIUS, who was younger than KEPLER, but for some time his cotemporary, taught more explicitly,† that the colours of bodies depend, not upon the light which is reflected from their anterior surfaces, but upon that portion of it which is received into their

* *Paralipomena in* VITELLIONEM, p. 23 et 436.

† *Optica Philosophia, Pars* I. p. 278 *et seq.*

internal parts, and is thence sent back through those surfaces. The following are some of the experiments, upon which he founded this doctrine. He exposed small round pieces of transparent glass, tinged with various colours, to the light of the sun, and received what was reflected from them upon white paper, in a darkened part of his room. He then found, that each glass produced two luminous circles, which, when the paper was sufficiently remote, were entirely separate from each other; and that the circle which proceeded from the upper surface of the glass was altogether without colour, while that which arose from the under surface, was of the same colour as the glass exhibited, when held between the light and the eye. From these experiments ZUCCHIUS also concluded, first, that every coloured body must be in some degree transparent, since a body absolutely impenetrable to light, could only reflect the colours of other bodies, but possess none of its own; and, secondly, that all bodies, which appear coloured when seen by reflected light, must be in some measure opake; for as the light which is reflected from their surfaces comes untinged to the eye, if that part of it which penetrates their substance were afterwards to proceed in it without impediment, no colour could be exhibited by them.*

* The works of ZUCCHIUS seem very little known, though they contain a considerable number of original experiments, and though it is probable that he was the inventor of the reflecting telescope. For he says (Pars I. p. 136.) it had occurred to him so early as 1616, that the same effect which is produced by the convex object-glass of a telescope, might be obtained by reflexion from a concave mirror; and that, after many attempts to construct telescopes with such mirrors, which proved fruitless from imperfections in their figure, he at length procured a concave mirror very accurately wrought, by means of which, and a *concave* eye-glass, he was enabled to prove his theory to be just. He does not mention at what precise time he constructed this

When Sir ISAAC NEWTON began his experiments upon light and colours, it was generally believed, that colours in opake bodies arise from some modification given to light, by the surfaces which reflect it. In opposition to one part of this opinion, our great philosopher maintained, that such bodies are seen coloured, from their acting differently upon the different colorific rays, of which white light is composed; but having established this point beyond dispute, he seems to have admitted, without inquiry, that colours are produced at the surfaces of the opake bodies to which they belong. For his experiments do not necessarily lead to such a conclusion; on the contrary, they are not more consistent with it, than they are with the opinion of KEPLER and ZUCCHIUS. This opinion, indeed, he appears not to have known; since he has taken for granted, what is contradicted by the experiments upon which it is founded, that the tinging particles of transparent bodies reflect coloured light.*

The very splendour of Sir ISAAC NEWTON's discoveries in optics, has probably done some injury to this branch of knowledge; for soon after they were made public, it became a common opinion, that the subject of light and colours had been exhausted by that great man, and that no writer upon it before him, was now worthy of being read. The former part of this opinion has long been generally acknowledged to be unjust; but the latter part of it is still maintained by many,

telescope; but his book was printed in 1652, eleven years before the publication of the "*Optica Promota*" of JAMES GREGORY. I have not met with any account of ZUCCHIUS, in MONTUCLA's or PRIESTLEY's histories; in the article "telescope," in the French Encyclopedia; or in any biographical dictionary which I have consulted.

* Optics, Book i. Part II. Prop. 10.

among whom may be placed the learned Mr. DELAVAL. This gentleman has lately published * a very elaborate treatise to prove, that the colours of opake bodies do not arise from the rays of light which they reflect from their anterior surfaces; but from that portion of it, which, having penetrated their anterior surfaces, is reflected by the opake particles which are diffused through their substance. But had the learned author not believed, that no European writer upon colours, before Sir ISAAC NEWTON, contained any valuable information upon that subject, he would probably have discovered, that both KEPLER and ZUCCHIUS had long ago maintained the very opinion which he now advances, and that they had built it upon experiments similar to his own. The merit of the invention of this theory belongs, therefore, to the great KEPLER: but still much praise is due to Mr. DELAVAL, both for reviving and confirming it; since, though it be not free from defects in some of its parts, it affords solutions of several optical difficulties, which, as far as I know, admit of an explanation from no other source. Among these I regard the phænomenon which is the subject of the present inquiry.

To shew then, from the theory of KEPLER, ZUCCHIUS, and DELAVAL, how common air and the neutral salts may brighten the appearance of blood, without producing any change upon its colouring matter, I shall first suppose that all its parts have the same reflective power. The consequence will be, that a mass sufficiently thick to suffocate the whole of the light which enters it, before it can proceed to the posterior surface, and be thence returned through the first surface, must appear black;

* Manchester Memoirs, Vol. II.

for the rays which are reflected from the first surface are without colour, and, by hypothesis, none can be reflected from its internal parts. In the next place, let there be dispersed through this black mass a small number of particles, differing from it in reflective power, and it will immediately appear slightly coloured: for some of the rays, which have penetrated its surface, will be reflected by those particles, and will come to the eye obscurely tinged with the colour, which is exhibited by a thin layer of blood, when placed between us and the light. Increase now by degrees the number of those particles, and in the same proportion as they are multiplied, must the colour of the mass become both stronger and brighter.

Having thus shewn that a black mass may become highly coloured, merely by a considerable reflexion of light from its internal parts; if I should now be able to prove, that both common air and the neutral salts increase the reflexion of light from the internal parts of blood, at the same time that they brighten it, great progress would certainly be made in establishing the opinion, that the change of its appearance, which is occasioned by them, depends upon that circumstance alone. But the following observations seem to place this point beyond doubt.

I compared several pieces of crassamentum of blood, which had been reddened by means of common air and the neutral salts, with other pieces of the same crassamentum, which were still black, or nearly so; upon which I found, that the reddened pieces manifestly reflected more light than the black. One proof of this was, that the minute parts of the former could be much more distinctly seen than those of the latter. Now this increased reflection of light, in the reddened pieces,

could not arise from any change in the reflective power of their surfaces; for bodies reflect light from their surfaces in proportion to their density and inflammability; and neither of those qualities, in the reddened pieces of crassamentum, can be supposed to have been augmented by common air, or a solution of a neutral salt in water. The increased reflection must, consequently, have arisen from some change in their internal parts, by means of which much of the light which had formerly been suffocated, was now sent back through their anterior surfaces, tinged with the colour of the medium through which it had passed.

The precise nature of the change which is induced upon blood by the neutral salts, is made manifest by the following experiment. I poured upon a piece of printed card as much serum, rendered very turbid with red globules, as barely allowed the words to be legible through it. I next dropped upon the card a little of a solution of nitre in water; when I observed, that, wherever the solution came in contact with the turbid serum, a whitish cloud was immediately formed. The two fluids were then stirred together; upon which the mixture became so opake, that the printed letters upon the card could no longer be seen. I have not hitherto been able to devise any experiment, which shews the exact change induced by common air; but it is evident that air must also, in some way, increase the opacity of blood, since it can, by no other means, increase the reflection of light from the interior parts of that body.

This theory explains another fact respecting the colour of blood, which might otherwise seem unaccountable. If a small quantity of a concentrated mineral acid be applied to a piece of dark crassamentum, the parts touched by it will for an instant

appear florid; but the same acids, added to a solution of the red matter in water, do nothing more than destroy its colour. Upon examining the crassamentum, a reason for this difference of effect is discovered; for the spots, upon which the acid was dropped, are found covered with whitish films. From which it seems evident, that the acid had occasioned an increase of opacity in the crassamentum, more quickly than it had destroyed its colour; and that the red matter, from having been in consequence seen by a greater quantity of light, had in that short interval appeared more florid than formerly.

The change which, I think, I have proved to take place in blood, when its colour is brightened by common air and the neutral salts, is similar to that which occurs to cinnabar, in the making of vermilion. This pigment, it is known, is formed from cinnabar, merely by subjecting it to a minute mechanical division. But the effect of this division is, to interpose among its particles, an infinite number of molecules of air, which, now acting as opake matter, increase the reflection of light from the interior parts of the heap, and by this means occasion the whole difference of appearance which is observed between those two states of the same chemical body.

I expect, however, it will be said, in opposition to what I have advanced, that, granting an increased reflection of light takes place from the interior parts of blood, in consequence of the application of common air and the neutral salts, still this is not a sufficient cause for the production of the colour which they occasion; for the colour of blood, after those substances have acted upon it, is a scarlet, which, agreeably to the observation of a learned and ingenious Fellow of this Society, Dr. G. FORDYCE,*

* Elements of the Practice of Physic, p. 13.

differs not only in brightness, but also in kind, from the ordinary colour of that fluid, which is a Modena red.

My answer is, that there are examples, beside that to which the objection is made, of dark blood appearing florid, merely from its colouring matter being seen by means of an increased quantity of light. One is afforded by rubbing a piece of the darkest crassamentum with a proper quantity of serum; for a mixture is thus formed, in a few seconds, possessing a colour similar to that which is given to crassamentum by common air. But here we certainly do nothing more, than interpose among the red globules a number of the less dense particles of serum; which, in their present situation, act as opake matter, and consequently increase the internal reflections. A second example occurs, when we view, by transmitted light, the fine edges and angles of a piece of crassamentum in water; for, in this situation, their colour appears to be a bright scarlet, though all the other parts of the same mass are black. These facts seem sufficient to prove, that the immediate cause I have assigned for the production of the florid appearance in blood, which has been exposed to the action of common air and neutral salts, is adequate to the effect; but I shall advance a step further, and shew how the Modena red is converted into a scarlet.

Blood, as I have found by experiment, is one of those fluids which Sir Isaac Newton has observed appear yellow,* if viewed in very thin masses. When, therefore, a number of opake particles are formed in it, by the action of common air and the neutral salts, many of them must be situated immediately beneath the surface. The light reflected by these will consequently be yellow; and the whole effect of the newly-

* Book i. Part II. Prop. 10.

formed opake particles, upon the appearance of the mass, will be the same, as if yellow had been added to its former colour, a Modena red. But Modena red and yellow are the colours which compose scarlet.*

I shall now relate the cautions to be observed in making the experiments, which are described in the beginning of this paper.

The first is, that the blood should be newly drawn, and the weather cool. For as the solution of the red matter is not to be filtred, but must become transparent by the gradual subsiding of whatever may render it turbid, if the blood be old, or the weather warm, it will often assume, before it be clear, a dark and purplish hue. When exposed in this state to the atmosphere in a broad and shallow vessel, its colour changes to a bright red, which, however, is not brighter than the proper colour of the solution. The dark purplish hue seems owing to some modification of sulphur; for the solution possessing it smells like hepatic air, particularly when agitated, and tarnishes silver which is held over it. Neutral salts produce no change upon this colour.

The second caution is, that the neutral salts be not added to the red solution, except when perfectly transparent; for if it be not so, the salts will render it more turbid, and the mixture will appear brighter, if seen by reflected light.

The last I shall note is, that the red solution ought to be poured gently from the vessel in which it has been made. If it be not, as it is a mucilaginous liquor, it is apt to entangle small particles of air, which by acting as opake matter, will for some time alter the appearance of the solution.

* FORDYCE's Elements of the Practice of Physic, p. 14.

I proceed next to offer a few observations upon the cause of the red colour of blood.

It has of late been very generally supposed, that blood derives its colour from iron. As far as I know, however, no other argument has been given in support of this opinion, than that the red matter is found to contain that metal. But there is certainly no necessary connection between redness and iron; since this metal exists in many bodies of other colours, and even in various parts of animals without colour, as bones and wool. More direct reasons, however, may be given for rejecting this opinion.

1. I know of no colour, arising from a metal, which can be permanently destroyed by exposing its subject, in a close vessel, to a heat less than that of boiling water. But this happens with respect to the colour of blood.

2. If the colour from a metal, in any substance, be destroyed by an alkali, it may be restored by the immediate addition of an acid; and the like will happen from the addition of a proper quantity of alkali, if the colour has been destroyed by an acid. The colour of blood, on the contrary, when once destroyed, either by an acid or an alkali, can never be brought back.

3. If iron be the cause of the red colour of blood, it must exist there in a saline state, since the red matter is soluble in water. The substances, therefore, which detect almost the smallest quantity of iron in such a state, ought likewise to demonstrate its presence in blood; but upon adding Prussian alkali, and an infusion of galls, to a very saturate solution of the red matter, I could not observe, in the former case, the slightest blue precipitate, or in the latter, that the mixture had acquired the least blue, or purple tint.

Upon the whole it appears to me, that blood derives its colour from the peculiar organization of the animal matter of one of its parts; for whenever this is destroyed, the colour disappears, and can never be made to return; which would not, I think, be the case, if it depended upon the presence of any foreign substance whatsoever.

I shall conclude this paper with relating several miscellaneous facts respecting the colour of blood, and some conclusions which may be formed from them.

Dr. PRIESTLEY has mentioned,* that the only animal fluid, beside serum, which he found to transmit the influence of common air to blood, was milk. But I have observed, that the white of an egg possesses the same property, notwithstanding its great tenacity. Now as serum contains an animal substance very similar to the white of eggs, it occurred to me as a question, whether, in transmitting the influence of air to blood, it acts by its salts only, or partly by means of the substance of which I have just spoken. I took therefore a quantity of urine, which is known to contain nearly the same salts as serum, and having added to it as much distilled water as rendered its taste of the same pungency as that of serum, I poured the mixture upon a piece of dark crassamentum of blood. I then put to another piece of the same crassamentum an equal quantity of serum, and exposed both parcels to the atmosphere. The result was, that the blood in the diluted urine did not become nearly so florid as that in the serum. I have found also, that a solution of sugar in water conveys the influence of air to blood; from which it seems probable, that milk owes its similar property to the saccharine matter which it contains. Black blood

* Phil. Trans. for 1776, p. 246.

exposed to the atmosphere under mucilage of gum arabic, does not become florid.

It has been said,* that neither serum, nor solutions of the neutral salts, dissolve the red matter of blood. But this induction has been made from too small a number of experiments. For saturate solutions of all the neutral salts, which I have tried, will extract, though slowly, red tinctures from blood, some of which are very deep; and neither they, nor serum, added in any proportion to a solution of the red matter in water, alter its colour or transparency, except by diluting it. The following experiments, however, will place this point in a clearer light.

I added a drachm of distilled water to an ounce of serum, and poured the mixture upon a small piece of crassamentum. Upon an equal piece of crassamentum I poured a drachm of water, and after some time added an ounce of serum. Each parcel, therefore, contained the same quantity of crassamentum, serum, and water; but the crassamentum upon which the mixture of serum and water had been poured, communicated no tinge to it; while the other piece, to which water had been first applied, and afterwards serum, gave a deep colour to the fluid above it. I made similar experiments with crassamentum, water, and a dilute solution of a neutral salt, which were attended with the same results.

Since then neither serum, nor a dilute solution of a neutral salt, will extract colour from blood, though they are both capable of dissolving the red matter, when separated by water from the other parts of the mass, it follows, in my opinion, that what are called the red globules consist of two parts, one

* Fordyce's Elements of the Practice of Physic, p. 14.

within the other, and that the outer, being insoluble in serum or dilute solutions of neutral salts, defends the inner from the action of those fluids. It is remarkable, that microscopical observations led Mr. HEWSON to the same conclusion, namely, that the red globules consist of two parts,* which, according to him, are an exterior vesicle, and an interior solid sphere. But the same writer, upon the authority of other microscopic experiments, asserts that the vesicles are red. If they be so, there must exist two red matters in the blood, possessing different chemical properties; which is certainly far from being probable.

The exterior part of the globule appears to be that ingredient of the blood upon which common air and the neutral salts produce their immediate effect, when they render the whole mass florid; for I have shewn they do not act upon the red matter itself, and I have not found that they occasion any change in coagulated lymph or serum. The only matter then which remains to be operated upon, is that which I have mentioned. It seems evident also, from what has been just stated, that there exists an animal matter in the blood, different from the coagulable lymph, the coagulable part of the serum, the putrescent mucilage, and the red particles, which, I believe, are all the kinds it has hitherto been supposed to contain.

The microscopical observations of Mr. HEWSON appear likewise to furnish a reason, why both water, and a saturate solution of a neutral salt, can extract colour from the red globules, though a mixture of those fluids be incapable of the same effect. For water applied to the red globules, separates the exterior vesicles from the red particles, which are therefore now

* HEWSON's Works, Vol. III. p. 17.

open to the action of any solvent.* The addition, however, of a small quantity of a neutral salt to the water enables the vesicles to preserve their shape, and to retain the inner spherules.† Upon the addition of a greater quantity of salt, the vesicles contract, and apply themselves closely to the red particles within.‡ Thus far Mr. Hewson's observations extend. Let it now be supposed that the vesicles contract still more, from a further addition of salt to the water; the consequence must be, that, as the internal particles are incompressible, the sides of the vesicles will be rent, and their contents exposed to the action of the surrounding fluid. Both water and a strong solution of a neutral salt may, therefore, destroy the organization of the vesicles, though in different ways, and thus agree in bringing the red matter in contact with a solvent; while a mixture of those two fluids, namely, a dilute solution of a neutral salt, will, by hardening the vesicles, increase the defence of the red matter against the action of such substances as are capable of dissolving it. But all reasoning founded upon experiments with microscopes, ought perhaps to be regarded as, in great measure, conjectural.

* Hewson's Works, Vol. III. p. 17. † Ibid. p. 40. ‡ Ibid. p. 31.

XX. *An Account of the Trigonometrical Survey, carried on in the Years 1795, and 1796, by Order of the Marquis* Cornwallis, *Master General of the Ordnance. By Colonel* Edward Williams, *Captain* William Mudge, *and Mr.* Isaac Dalby. *Communicated by the Duke of* Richmond, *F. R. S.*

Read May 11, 1797.

PART FIRST.

PREAMBLE.

According to the resolution expressed in the account of the Trigonometrical Survey, printed in the Philosophical Transactions for the year 1795, we now communicate to the public, through the same channel, a farther relation of its progress.

On referring to the above paper, it will be found that, for the prosecution of this undertaking, a design was formed of proceeding to the westward, with a series of triangles, for the survey of the coast. This intention has been carried into effect; and as the small theodolite, or circular instrument, announced in our former communication as then in the hands of Mr. Ramsden, was finished early in the summer of 1795, we are enabled to give a series of triangles, extending, in conjunction with those before given, from the Isle of Thanet, in Kent, to the Land's End.

In the composition of the following account, we have adhered to the plan adopted in the last, of giving the angles of

the great triangles, with their variations; and we have, with as much brevity as possible, inserted a narrative of each year's operations. This will be found, however, to extend only to the First Part, or that containing the particulars of the survey in which the great instrument alone was used. The remaining contents of this portion of the work, are necessarily confined to the angles of the principal, and secondary triangles, with the calculations of their sides, in feet; and likewise such *data* as have no connection with the computations of latitudes and longitudes.

Part the Second contains an account of a survey carried on in Kent, in the years 1795 and 1796, with the small instrument, by order of the Master General, for completing a map of the eastern and southern parts of that county, for the use of the Board of Ordnance, and the military commanders on the coast.

In Part the First will be found an article, for which we are indebted to Dr. MASKELYNE, the Astronomer Royal. It contains his demonstration of M. DE LAMBRE's formula, in the *Connoissance des Temps* of 1793, for reducing a distance on the sphere to any great circle near it, or the contrary. The *practical rule* thence derived, for reducing the angles in the plane of the horizon, to those formed by the chords, is very useful, and will considerably abridge the trouble which must necessarily arise in computing the chord corrections by any former method.

SECTION FIRST.

ARTICLE I. *Of Particulars relating to the Operations of the Year 1795.*

In an early part of this season, from the necessity which existed of completing the map of Kent, mentioned in the preamble, we had conceived that our former intentions, of continuing the survey towards the west, would for the present be relinquished; as it was not imagined that the telescope of the small circular instrument, then in the hands of Mr. RAMSDEN, could be applied, with good effect, in observing staffs erected on very distant stations.

From the obvious importance, however, of adhering to the first resolution, it was determined that a trial should be made of the excellence of this instrument, in the construction of which extraordinary pains had been taken, by operating with it in Kent, and using it for those purposes to which, if the object before spoken of had not been in view, the great theodolite would have been necessarily applied.

This smaller theodolite, therefore, as a substitute, was in May taken into Kent by Mr. DALBY, and Mr. GARDNER, chief draughtsman in the Tower; the assistance of the former being necessary, as the stations in the series of 1787 were for the most part unknown to the latter gentleman.

As the former paper, relating to the trigonometrical survey, could not be presented to the Royal Society before the 4th of June, the business did not commence till the 12th of the same month. The party then left London, and the instrument was taken to Bull Barrow, in Dorsetshire.

On a reference to the account of 1795, it will be seen, that a station was chosen near Lulworth, and observed both from Nine Barrow Down and Black Down. It was also intended to be observed from Bull Barrow; by which means the great triangle, formed by the stations Black Down, Nine Barrow Down, and Bull Barrow, would be divided into, and made to consist of, two smaller triangles. This, however, it was now found could not be done, as a signal house had been erected near the station at Lulworth, subsequent to the operations in 1794, which prevented that spot from being afterwards seen at Bull Barrow: but no consequences very injurious can have arisen from the impracticability of making use of this station in the manner originally proposed, since the stations formerly chosen in Portland, with which that of Lulworth was also intended to connect, have not been visited with the instrument. The stations in that island were selected with a view of observing from them, and Charton Common, some point in the vicinity of Torbay, which might be a proper station in the series intended to be carried along the coast. Such a situation, however, could not be conveniently found, as the view of Devonshire from Charton Common is much interrupted by trees and other obstacles; and it would have been highly improper to shorten the side between Pilsden Hill and the coast, by choosing a station more remote from the latter than Charton Common.

As from an inspection of the plan of the triangles annexed to this account, a doubt may be entertained as to the propriety of carrying on so very extensive a series from the short side connecting the stations on Black Down and Mintern Hill; it must be observed that, admitting the necessity of adopting Bull Barrow for a station, those on Pilsden and Mintern Hills were

naturally chosen; the first, because it connected with Dumpdon (a station that could not be dispensed with); and the second, because it was the point most remote from Black Down, being on the brow of the high land overlooking the general surface of Somersetshire.

To connect with the station formerly chosen near Maiden Bradley, two others were selected whilst the party were at Bull Barrow; one on Ash Beacon, near Sherborne, and the other on the Quantock Hills. Both these have very commanding views, and will hereafter easily unite with any stations which may be chosen to the northward.

From Bull Barrow, the instrument was successively taken to the following stations, before any other new ones were chosen, *viz.* Mintern, Pilsden, and Charton Common; and whilst the party were at the latter, nearly all the stations were selected in Devonshire. In the choice of these, much difficulty occurred, as the face of this county is particularly unfavourable for operations of this kind. Around Honiton and Chard, there are several small ranges of hills, nearly of an equal height, running in parallel directions. Near the former are three, thus circumstanced; *viz.* Hembury Fort, Combe Raleigh, and Dumpdon. From the first and second of these, the station on Charton Common is not visible; and it is from the last only, that both Pilsden and the Quantock Hills can be seen. This station, however, has a disadvantage: Combe Raleigh, which is to the west of it, takes off all view round Tiverton and Silferton; so that it became indispensably necessary to select a spot on the northern extremity of Dartmoor, called Cawsand Beacon.

To those who are acquainted with the interior of Dartmoor,

it will be unnecessary to assign the reason for not having chosen any station towards its centre. It may be sufficient to observe, that two spots were found on its circumference, which render the want of it trifling in its consequences.

Independent of the stations to which, as we have before observed, the instrument was taken this year, the following were visited, *viz.* Dumpdon, Little Haldon, Furland, and Butterton. From the latter, the party returned to London in the month of October.

ART. II. *Angles taken in the Year* 1795.

At Bull Barrow.

Between	°	′	″		Mean.
Mintern Hill and Black Down -	46	54	33		″
			34,75	}	34
			34		
Black Down and Nine Barrow Down	84	31	22,25	}	23,25
			24		
Nine Barrow Down and Wingreen	93	33	0,5	}	0,25
		32	59,75		

At Mintern Hill.

Bull Barrow and Black Down -	101	39	30	}	30,5
			31,25		
Black Down and Pilsden - -	68	30	45,75	}	46,5
			47		

On Charton Common.

Little Haldon and Dumpdon -	68	12	49,75	}	51,25
			51,25		
			52,75		
Dumpdon and Pilsden - -	93	54	36,25	}	37,25
			37,5		
			38		

Between	°	′	″	Mean.
Pilsden and Black Down - -	47	39	17,5 19,25	18,5
On Pilsden Hill.				
Mintern and Black Down -	44	37	51,5 52,5 53 54,25 55,5	53,25
Black Down and Charton Common	105	5	25,75 26 26	26
Charton Common and Dumpdon	47	32	0,25 1,25 2,5	1,25
At Dumpdon.				
Charton Common and Little Haldon	86	39	7 7,25 8,5 8,75 9,25	8,25
Little Haldon and Cawsand Beacon	35	7	6,5 6,75 8,25	7,25
Pilsden and Charton Common -	38	33	22 22,25 22,25 23 23,5 23,5	22,75
At Little Haldon.				
Furland and Rippin Tor - -	84	58	42 43	42,5

Between	°	′	″	Mean.
Rippin Tor and Cawsand Beacon	29	30	9,25	″
			11	10,5
			11	
Dumpdon and Charton Common	25	8	0,75	1,25
			2	
Dumpdon and Furland -	148	52	32,75	
			33	33,25
			34	
At Furland.				
The Bolt Head and Butterton -	53	15	34,25	35
			35,75	
Butterton and Rippin Tor -	48	38	4	4,5
			5,25	
Rippin Tor and Little Haldon -	39	24	36,75	37,25
			37,75	
At Butterton.				
Rippin Tor and Furland - -	74	21	56	
			56,5	
			57,25	57,25
			58	
			58,5	
Furland and the Bolt Head -	68	47	50,75	50,75
			50,75	
The Bolt Head and Kit Hill -	127	37	36,5	36,5
			36,75	
Maker Heights and Kit Hill -	42	11	38,75	38,75
			38,75	
Maker Heights and Carraton Hill	35	30	28	
			28,75	28,75
			29,75	

ART. III. *Of Particulars relating to the Operations of the Year 1796.*

In the account of this Survey, published in the Philosophical Transactions for 1795, page 473, it is stated, that large stones were sunk in the ground at the extremities of the base of verification on Salisbury Plain. To render these points permanent, two iron cannon (selected from among the unserviceable ordnance in Woolwich Warren) were, towards the end of February, sent to Salisbury, and in the beginning of March inserted at the ends of the base. The same methods were adopted, for the purpose of fixing these cannon in their proper positions, as those made use of when similar *termini* were sunk in the ground on Hounslow Heath. This operation having been completed on the 10th of March, the instrument was shortly after carried to Kit Hill, in Cornwall; a station, like that on Bindown, chosen rather for the purpose of a secondary, than a principal place of observation.

It would be tedious, and perhaps unnecessary, to enumerate the names of all the stations selected this year, as many of them do not form any part of the series now given to the public. We shall, therefore, confine ourselves to such remarks on the subject as may serve to abridge this article.

We have before stated, that a station was chosen on Cawsand Beacon, the northern extremity of Dartmoor, for the purpose of connecting with Dumpdon. It should have been observed, that to the westward of the former eminence, and near it, there is a hill considerably higher, which in point of situation has many advantages, but which cannot be made

use of on account of the ruggedness of its surface, which seems to render the carrying of the instrument to its top almost impossible. From this circumstance, and similar impediments, which the high lands remote from the circumference of Dartmoor offer to our operations, it results, that the body of this moor cannot have any great triangles carried over it: such stations were therefore selected this year as may serve, in conjunction with others, to include this tract of country in a polygon of a small number of sides.

To make observations for the purpose of hereafter determining the longitude and latitude of the Lizard, was a principal object in this year's operations; and as this headland seems to offer itself as very convenient for a station, it will be right to assign our reasons for not having chosen one upon it.

As no other spot but Hensbarrow Beacon could be found in that part of Cornwall proper for a station, it became necessary to fix on the Deadman, or Dodman, for another point in the series. From this place no part of the land within four miles of the Lizard can be seen, as the high ground about Black Head, which is to the eastward of the latter, is nearly in a line between them, and is also much higher than both. It will be perceived, however, that no evil can result from the want of such a station, as the light-houses and the naval-signal-staff at the Lizard, have been intersected from several stations. The precise spot on which Mr. BRADLEY made his observations in the year 1769, for ascertaining the longitude and latitude of this headland, was pointed out by the person having the care of the light-houses, who well remembered the common particulars relating to his operations: such measurements were made from the light-houses to this spot, as may enable us, at a future

period, to compare the results from the *data* afforded by the trigonometrical operation, with those deduced from the astronomical observations made by the above gentleman. It may be also mentioned, that angles were at the same time taken at the western light-house and signal-staff, for the purpose of finding the situation of the Lizard Point.

We are now to speak of the most important business performed this year; that of making observations to determine the distance of the Scilly Isles from the Land's End.

To do this as accurately as possible, it became necessary to find stations affording the longest *base*. The hill near *Rosemergy*, called the *Watch*, and the station near St. Buryan, are certainly the most advantageous places, because all the islands can be seen from both; but we could not avail ourselves of the former, as difficulties almost insuperable would have attended an attempt to get the instrument upon it. Another station was therefore selected, on Karnminnis, near *St. Ives;* a spot as well situated as the place spoken of, provided all the islands could be seen: this, however, does not prove to be the case, *St. Martin's Day-Mark* being the only object in the Scilly Islands visible from Karnminnis.

From the stations near the Land's End (Sennen and Pertinney), as well as that above mentioned (St. Buryan), St. Agnes' Light-house, and two objects in St. Mary's, were observed; and as the means by which all their distances are determined, except those of the Day-Mark, from the shortness of the bases (which were, however, the longest that could be found) are exceptionable, it will be right to mention, that while we were engaged in that part of the operation now spoken of, the air was so unusually clear, that we could sometimes, with the

telescope of the great theodolite, discover the soldiers at exercise in St. Mary's island.

Under this article, it will be convenient to state, that we have endeavoured to find some spot to the westward, on which a base might be measured. Had we been fortunate in this respect, it undoubtedly would be eminently advantageous; as those triangles, now extended to the Land's End, would, in that case, be verified in some part of the new series. In Devonshire and Cornwall, however, no place has been discovered by any means fit for the purpose; so that our communicating this work, under the circumstances attending it, is a matter of necessity.

In the present and former seasons, such stations were selected and observed, as were judged to be proper for the future use of the small instrument; and as we had experienced, in the early stage of this Survey, much delay and disappointment from the white lights not being always seen when fired on distant stations, we have since substituted lamps and staffs in their stead. The operations of the present year were continued till October, when the party returned to London.

ART. IV. *Angles taken in the Year* 1796.

At Kit Hill.

Between		°	′	″	Mean.
Butterton and Maker Heights	-	48	36	45 47,75	46,5
Maker Heights and Bindown	-	53	21	18,75	
Carraton Hill and Bindown	-	50	45	31	

On Maker Heights.

Between	°	′	″	Mean.
Lansallos and Carraton Hill -	48	39	54,75	54,75
			54,75	
Carraton and Butterton -	112	18	7,75	8,75
			9,75	
Butterton and the Bolt Head -	45	54	35,75	37
			38,5	
Bindown and Carraton Hill -	28	22	50,75	
Bindown and Kit Hill - -	51	29	20,5	22,5
			24,5	
Kit Hill and Butterton - -	89	11	33,25	34,75
			36	

At the Bolt Head.

Maker Heights and Butterton -	48	39	24,5	24,75
			24,75	
Butterton and Furland - -	62	56	36,5	

At Rippin Tor.

Cawsand Beacon and Little Haldon	124	59	12,75	13
			13,5	
Little Haldon and Furland -	55	36	39	40,5
			41,75	
Furland and Butterton - -	61	59	59,25	59,5
			59,5	

On Cawsand Beacon.

Dumpdon and Little Haldon -	48	14	20	21,25
			22,5	
Little Haldon and Rippin Tor -	25	30	39,5	39,75
			40,25	

On Carraton Hill.

Between	°	′	″	Mean.
Maker Heights and Lansallos -	67	19	20,25	21,75
			23,5	
Lansallos and Bodmin Down -	56	21	16,75	17
			17	
Lansallos and Hensbarrow Beacon	87	28	57,75	58
			58	
Butterton and Maker Heights -	32	11	22,5	23
			23,5	
Kit Hill and Bindown - -	91	45	22,5	
Maker Heights and Bindown -	38	58	38,5	

On Bindown.

Lansallos and Carraton Hill -	119	9	36,25	
Carraton Hill and Kit Hill -	37	29	5,75	
Kit Hill and Maker Heights -	75	9	24,5	

At Lansallos, or Polvinton Farm.

Deadman and Hensbarrow Beacon	52	34	2	3
			2,5	
			5	
Hensbarrow Beacon and Bodmin Down	45	1	10,75	11,75
			12,75	
Bodmin Down and Carraton Hill	54	57	43,25	44
			44,75	
Carraton Hill and Bindown -	32	36	43,25	
Carraton Hill and Maker Heights	64	7	43,5	44,25
			43,75	
			45,75	

On Bodmin Down.

Carraton Hill and Lansallos -	68	40	57,75	59
		41	0,75	
		40	58,5	

Between	°	′	″	Mean.
Lansallos and Hensbarrow Beacon	67	59	27,5 28	27,75
On Hensbarrow Beacon.				
Carraton Hill and Lansallos -	42	32	8,5	
Bodmin Down and Lansallos -	66	59	21,75 25	23,25
Lansallos and Deadman - -	71	18	35 35,25 35,5	35,25
Deadman and St. Agnes' Beacon	77	20	28,5 28,75 31,5	29,5
On St. Agnes' Beacon.				
Hensbarrow Beacon and Deadman	84	31	17 21 23	20,25
Deadman and Karnbonellis -	75	51	53 53,75	53,25
Karnbonellis and Karnminnis -	57	46	31 31,5	31,25
On Karnminnis.				
St. Agnes' Beacon and Karnbonellis	32	30	0,25 02,5	0,25
Karnbonellis and St. Buryan -	111	53	15,5 16,5	16
St. Buryan and Pertinney -	18	48	16,75 17 20,75	18
At St. Buryan.				
Karnminnis and Karnbonellis -	41	43	45,25 45,5 45	45,25

Between	°	′	″	Mean.
Pertinney and Karnminnis -	52	31	27,5	27,5
			27,5	
Sennen and Pertinney - -	75	36	11	11,5
			11,75	
			12	
At Sennen.				
Pertinney and St. Buryan -	36	39	18,5	18,75
			19,25	
On Pertinney.				
Karnminnis and St. Buryan -	113	40	15,25	15,5
			16	
St. Buryan and Sennen - -	67	44	30,5	31
			31,25	
At Karnbonellis.				
St. Buryan and Karnminnis -	26	29	59,25	59,25
			59,5	
Karnminnis and St. Agnes' Beacon	89	43	27,25	29
			28,75	
			31,25	
St. Agnes' Beacon and the Deadman	78	16	39,75	41
			40,5	
			48	
On the Deadman, or Dodman Point.				
Karnbonellis and St. Agnes' Beacon	25	51	24,5	24,75
			24,75	
St. Agnes' Beacon and Hensbarrow Beacon	68	8	12,5	13
			13,75	
Hensbarrow Beacon and Lansallos	56	12	22,5	22,75
			22,75	

ART. V. *Situations of the Stations.*

Mintern, or *Revel's Hill.* This station is in Dorsetshire, and situated on Revel's Hill, which is not far from Mintern. It is 17 feet N. E. from the corner of the hedge.

Pilsden. This station is also in Dorsetshire, and near Broadwindsor. The point is on the S. E. corner of the old parapet.

Charton Common. The station is in the field adjoining to, and also to the westward of, the Common, and is about two miles from Lyme: it is 50 yards from the eastern hedge, and may be easily found, as Black Down is only visible from that spot, being seen between two trees.

Dumpdon; about three miles N. E. of Honiton. The station is 10 feet northward of the hedge of the plantation, and nearly on the highest part of the hill.

Little Haldon; near Teignmouth, in Devonshire. The station is 80 yards from the *Direction Post*, and in a line with it and the Obelisk on *Great Haldon*.

Cawsand Beacon; near South Zeal. The station is about 200 feet north of the Karn, or great heap of stones.

Rippin Tor. This station is also on Dartmoor, and about 5 miles from Ashburton. The point is mid-way between the two heaps of stones.

Furland; a field near the turnpike-gate between Brixen and Dartmouth. The station is near the stone, erected in the middle of the field.

Butterton. The station is 45 feet S. W. of the Karn, on the hill called by this name, and about 1 mile from Ivy Bridge.

The Bolt Head. The station is on the spot called *White Soar*, above the Bolt; it is 95 feet in the line produced, north-

ward, from the west side of the signal-house, and about 90 feet from the nearest corner of it.

Maker Heigbts. This spot is near Cawsand, and the station is 45 feet from the great flag-staff, in the line produced from Statten Battery passing by the side of the staff.

Kit Hill, near Callington. The station is on the S. W. bastion of a work, similar to an Indian fortification.

Carraton Hill. This station is about 4 miles north of Liskeard; and the point 130 yards south of the highest Karn on the top of the hill.

Bindown, near Looe. The station is 50 yards eastward of the barrow on this hill.

Lansallos. The station is in a field belonging to *Polvinton Farm*, which is near that town. The point is 159 feet from the western bank, and 90½ from the southern one.

On Bodmin Down. The station 120 yards south of the high road, and about a quarter of a mile east of the turnpike gate. The point is in the centre of a remarkable ring.

Hensbarrow Beacon, near St. Roach. The station is on the top of the barrow.

The Deadman, or *Dodman Head.* The station is about 40 feet south of the bank, and nearly 100 yards to the east of the entrance into the inclosure.

St. Agnes' Beacon. The station is on the southern brow of the beacon, and about 80 yards from the tower.

Karnbonellis. The station is 90 yards south of the northern Karn, or heap of stones. The hill called *Karnbonellis* is near *Poreillis.*

Portinney. The station is in the middle of the ring on its top. This hill is about 2 miles eastward of *St. Just.*

Sennen. This station is in the north-west corner of a field belonging to Mr. WILLIAMS. The field may be easily found, as there is no other spot near the town of *Sennen*, from which the Longship's Light-house, Pertinney, and St. Buryan, can be seen.

Karnminnis, near St. Ives. The station on the top of this hill, may be found from the following measurements:

	Feet.	In.		
The station from 3 large moor-stones, south of the hedge.	8	8	from the south	stones.
	11	0	—— north	
	14	1	—— west	

St. Buryan. The station is in a field adjoining the town, and by the side of the *Penzance* road. It is $84\frac{1}{2}$ feet from the stile, and 48 feet from a large stone in the northern hedge. This stone is 81 feet from the stile; the station, this stone, and Chapel Karnbury, being in a right line.

ART. VI. *Demonstration of M.* de Lambre's *Formula in the Connoissance des Temps of* 1793, *for reducing a Distance on the Sphere to any great Circle near it, or the contrary. By* Nevil Maskelyne, *D. D. F. R. S. and Astronomer Royal.*

Put A = angle subtended by two terrestrial objects; a = the same reduced to the horizon; H, b the two apparent altitudes: if either is a depression, it must be taken negative.

By spherics, c, $A = c, a \,.\, c, H \,.\, c, b + s, H \,.\, s, b$.

Put $A = a + d\,a$, where $d\,a$ signifies $A - a$, and not their differential.

By trigonometry c, $A = c, a \,.\, c, d\,a - s, a \,.\, s, d\,a = c, a \times 1 - \overline{vs, d\,a} - s, a \,.\, s, d\,a = c, a - c, a \times 2\,s^2, \frac{1}{2}\,d\,a - s, a \,.\, s, d\,a$ (by theo-

rem above) $= c, a \,.\, c, \mathrm{H} \,.\, c, b + s, \mathrm{H} \,.\, s, b \cdots s, d\,a + 2\, s^3$,
$\frac{1}{2} d\,a \,.\, {}'t, a = {}'t, a - {}'t, a \,.\, c, \mathrm{H} \,.\, c, b - s, \mathrm{H} \,.\, s, b \times$ cosec. a
$= t', a - {}'t, a \times \overline{\frac{1}{2} c, \overline{\mathrm{H} - b} + \frac{1}{2} c, \overline{\mathrm{H} + b}} -$ cosec. a
$\times \overline{\frac{1}{2} c, \overline{\mathrm{H} - b} - \frac{1}{2} c, \overline{\mathrm{H} + b}}$ (because $t', a = \frac{1}{2}\,{}'t, \frac{1}{2} a - \frac{1}{2} t, \frac{1}{2} a$;
and cosec. $a = \frac{1}{2}\,{}'t\,\frac{1}{2} a + \frac{1}{2} t, \frac{1}{2} a) = \overline{\frac{1}{2} t', \frac{1}{2} a - \frac{1}{2} t, \frac{1}{2} a}$
$\times \overline{1 - \frac{1}{2} c, \overline{\mathrm{H} - b} - \frac{1}{2} c, \overline{\mathrm{H} + b}} - \overline{\frac{1}{2}\,{}'t, \frac{1}{2} a + \frac{1}{2} t, \frac{1}{2} a}$
$\times \overline{\frac{1}{2} c, \overline{\mathrm{H} - b} - \frac{1}{2} c, \overline{\mathrm{H} + b}} = \frac{1}{2}\,{}'t, \frac{1}{2} a \times \overline{1 - c, \overline{\mathrm{H} - b}}$
$- \frac{1}{2} t, \frac{1}{2} a \times \overline{1 - c, \overline{\mathrm{H} + b}} = \frac{1}{2}\,{}'t, \frac{1}{2} a \times vs, \overline{\mathrm{H} - b} - \frac{1}{2} t, \frac{1}{2} a$
$\times vs, \overline{\mathrm{H} + b} = {}'t, \frac{1}{2} a \,.\, s^3, \frac{1}{2} \overline{\mathrm{H} - b} - t, \frac{1}{2} a \,.\, s^3, \frac{1}{2} \overline{\mathrm{H} + b}.$

Put $n = {}'t, \frac{1}{2} a \,.\, s^3, \frac{1}{2} (\mathrm{H} - b) - t, \frac{1}{2} a \,.\, s^3, \frac{1}{2} (\mathrm{H} + b)$.

We shall have

$s, d\,a + 2\, s^3, \frac{1}{2} d\,a \,.\, {}'t, a = n$;

and $s, d\,a = n - 2\, s^3, \frac{1}{2} d\,a \,.\, t', a$.

But $s, d\,a = 2\, s, \frac{1}{2} d\,a \,.\, c, \frac{1}{2} d\,a$

$\therefore s, \frac{1}{2} d\,a = \frac{s, d\,a}{2\, c, \frac{1}{2} d\,a} = \frac{n - 2\, s^3, \frac{1}{2} d\,a \,.\, {}'t, a}{2\, c, \frac{1}{2} d\,a}$,

and $s, d\,a = n - 2\, s^3, \frac{1}{2} d\,a \,.\, t', a = n - 2\, t', a \left(\frac{n - 2\, s^3, \frac{1}{2} d\,a \,.\, {}'t, a}{2\, c, \frac{1}{2} d\,a} \right)^3$,

because $\left. \frac{n - 2\, s^3, \frac{1}{2} d\,a \,.\, t\,a}{2\, c, \frac{1}{2} d\,a} \right|^3 = \frac{n - 4\, n \,.\, s^3, \frac{1}{2} d\,a \,.\, t', a + 4\, s^6 \,.\, \frac{1}{2} d\,a \,.\, {}'t', a}{4 \times 1 - s^3, \frac{1}{2} d\,a}$

$= \frac{n^3}{4} + \frac{n^3\, s^3, \frac{1}{2} d\,a \,.}{4} - n \,.\, s^3, \frac{1}{2} d\,a \,.\, {}'t, a - n \,.\, s^3, \frac{1}{2} d\,a \,.\, {}'t, a$
$+ s^3, \frac{1}{2} d\,a \,.\, {}'t', a) = n - \frac{1}{2} n^3 \,.\, t', a - \frac{1}{2} n^3 \,.\, {}'t, a \,.\, s^3, \frac{1}{2} d\,a$
$+ 2\, n \,.\, {}'t', a \,.\, s^3, \frac{1}{2} d\,a + 2\, n\, {}'t', a \,.\, s^3, \frac{1}{2} d\,a - 2\, {}'t', a \,.\, s^3 + \frac{1}{2} d\,a,$

by substituting for $s, \frac{1}{2} d\,a$ its near value n,

$= n - \frac{1}{2} n^3\, t', a - \frac{n^3\, t', a}{8} + \frac{1}{2} n^3\, t', a + \frac{1}{8} n^3\, {}'t', a - \frac{1}{8} n^4\, {}'t', a,$

where the last term but one containing the 5th power of n may be rejected, as it has been omitted by M. DE LAMBRE.

As $d\,a$ is always very small, the arc $d\,a$ in parts of the radius, unity, $= s, d\,a$ in parts of the same radius, therefore

$s, 1'' : 1'' :: s, d\,a$ (in parts of radius unity) : $\frac{1}{s,\,1''} \times s,\, d\,a = d\,a$ in seconds,

$= \frac{1''}{s,\,1''} \times n - 2\,s^2, \frac{1}{2}\,d\,a\,.\,'t, a = \frac{1''}{s,\,1''} \times \overline{n - d\,a\,.\,s, \frac{1}{2}\,d\,a\,.\,'t, a} = \frac{1'' \times n}{s,\,1''}$

$- \frac{1'' \times d\,a\,.\,s, \frac{1}{2}\,d\,a\,.\,'t, a}{s,\,1''}$ . . . if we put $n = \frac{1'}{s,\,1''} \times t', \frac{1}{2}\,a\,.\,s, \frac{1}{2}\,(\mathrm{H} - b)$ $- t, \frac{1}{2}\,a\,.\,s^2, \frac{1}{2}\,(\mathrm{H} + b)$, and $d\,a =$ a number of seconds, we shall have

$d\,a = n - d\,a\,.\,s, \frac{1}{2}\,d\,a\,.\,'t, a$; and, for the most part, without any sensible error, $d\,a = n - n\,.\,s, \frac{1}{2}\,n\,.\,'t, a$.

Table I. contains $\frac{1'' \times t, \frac{1}{2}\,a}{10000}$, and $\frac{1'' \times 't, \frac{1}{2}\,a}{10000}$; Table II. contains $10000 \times s^2, \frac{1}{2}\,(\mathrm{H} \mp b)$. Table III. contains the term $- n\,.\,s, \frac{1}{2}\,n\,.\,'t, a$. The argument on the side is a, and that on the top is n or the result found by the help of the two first tables. If this correction should be considerable, with the value of $d\,a$, found after this correction has been applied, enter Table III. again at the top, and with a on the side as before; the number now found subtracted from n will give the correct value of $d\,a$.

By the investigation,

$d\,a = \frac{1}{2}\,'t, \frac{1}{2}\,a\,.\,vs\,\overline{\mathrm{H} \simeq b} - \frac{1}{2}\,t, \frac{1}{2}\,a\,.\,vs, \overline{\mathrm{H} \pm b} - vs, d\,a\,.\,'t\,a$,

where the upper or lower signs are to be used, according as the objects are on the same, or on contrary sides of the great circle to which they are referred; the third term will be negative or positive, according as a is less or more than 90°.* If $d\,a$ should come out negative, A will be less than a, or a greater than A. In the case of reducing a spheric angle to the angle

* Compute the two, which will give the approximate value of $d\,a$, and make use of them in computing the third term; and join the three terms together according to their signs, which will give $d\,a$ still nearer; and, if this should prove considerable, compute the third term a second time with the new value of $d\,a$.

between the chords, the spheric angle will be represented by a, and the angle between the chords by $A = a + da$; and $da = \frac{1}{2}'t, \frac{1}{2}a \,.\, vs, \overline{H \sim b} - \frac{1}{2}t, \frac{1}{2}a \,.\, vs, \overline{H + b} - vs, da \,.\, 't, a$ (if D, d represent the arcs to the chords) $= \frac{1}{2}'t, \frac{1}{2}a \,.\, vs, \frac{1}{2}(D \sim d) + \frac{1}{2}t, \frac{1}{2}a \,.\, vs, \frac{1}{2}(D + d) - vs, da \,.\, 't, a$;

$A = a - (\frac{1}{2}t, \frac{1}{2}a \,.\, vs, \frac{1}{2}\overline{D + d} - \frac{1}{2}'t, \frac{1}{2}a \,.\, vs\, \frac{1}{2}D \sim d) - vs, da \,.\, 't, a$; where the last term will change its sign to affirmative, if a is greater than 90°. If the answer is required in seconds, the correction must be multiplied by 206265, the number of seconds in an arc = radius. The calculation will be easily made by logarithms.

Practical Rule.

The practical rule deduced from the above conclusions is the following, and given in the words of the Astronomer Royal.

"To the constant logarithm 5,0134 add L . t, $\frac{1}{2}a$ and L . "$vs\,\overline{D + d}$; the sum diminished by 20 in the index is the "logarithm of the first part of the value of da in seconds, "which is always negative. To the constant logarithm 5,0134 "add L . t', $\frac{1}{2}a$, and L . vs, $\frac{1}{2}\overline{D \sim d}$, the sum diminished by 20 "in the index, is the logarithm of the second part in seconds, "which is always affirmative. These two joined together, ac- "cording to their proper signs, will give the approximate value "of da. To its logarithmic versed sine, add L . t', a and con- "stant logarithm 5,8144, the sum, diminished by 20 in the "index, will be the logarithm of the third part in seconds, "which will be negative or affirmative, according as a is less "or more than 90°. This applied according to its sign, to the

" approximate value of $d\,a$, will give the correct value of $d\,a$.
" If the third part comes out considerable, it should be com-
" puted anew with the last value of $d\,a$. The value of $d\,a$,
" finally corrected, applied to a, will give A, the angle between
" the chords."

In the application of the above rule, to the computation of such corrections as may be applied to the angles of any triangles in this survey, it is manifest that the last step may be entirely neglected on account of the smallness of the *approximate* value of $d\,a$, whose versed sine is one of the arguments. Being, therefore, confined to the use of the two first steps, the operation is very short. An example is here given in the computation of the correction for reducing the angle at Chanctonbury Ring in the 39th triangle, given in the last account (see Phil. Trans. for 1795, p. 492), to that formed by the chords.

EXAMPLE.

Constant logarithm - -	5,0134	- - - - - -	5,0134
Log. tang. $\frac{1}{2}a = 78° 56'$ -	10,7112	Log. co. tang. $\frac{1}{2}a$ - -	9,2887
Log. $vs.\,\frac{1}{2}.\overline{H+b} = 19° 53'',5$	5,2237	Log. $vs.\,\frac{1}{2}\overline{H-b} = 5'\,53'',5$	4,1669
	0,9483 + 8'',88		− 2,4690 + 0'',03
1st correction − 8 ,88			
2d correction + 0,03			
− 8,85 the correction required.			

SECTION SECOND.

Calculation of the Sides of the great Triangles, carried on from the Termination of the Series, published in the Philosophical Transactions of the Year 1795, along the Coasts of Dorsetshire, Devonshire, and Cornwall, to the Land's End.

Distance from Wingreen to Nine Barrow Down, 130224,5 Feet (see Phil. Trans. for 1795).

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
XLIII.	Wingreen -	54 29 36,5	—0,4			54 29 36	
	Bull Barrow -	93 33 0,25	—0,91			93 32 59	
	Nine Barrow Down	31 57 25,5	—0,4			31 57 25	
		180 0 2,25		1,72	+0,53		
	Bull Barrow from { Wingreen - -						69058
	Bull Barrow from { Nine Barrow Down -						106213
XLIV.	Black Down -	56 30 18,75	—0,53			56 30 18,5	
	Nine Barrow Down	38 58 19,25	—0,89			38 58 19	
	Bull Barrow -	84 31 23,25	—0,57			84 31 22,5	
		180 0 1,25		1,99	—0,74		
	Black Down from { Nine Barrow Down -						126782
	Black Down from { Bull Barrow - -						80103,8
XLV.	Mintern -	101 39 30,5	—0,36			101 39 30	
	Bull Barrow -	46 54 34	—0,09			46 54 33,5	
	Black Down -	31 25 57,5	—0,11			31 25 56,5	
		180 0 2		0,59	+1,41		
	Mintern from { Bull Barrow - -						42653,4
	Mintern from { Black Down - -						59730.

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
XLVI.	Pilsden	44 37 53,25	—0,29			44 37 53	
	Mintern Hill	68 30 46,5	—0,36			68 30 40	
	Black Down	66 51 21,25	—0,36			66 51 21	
		180 0 1		1	— 0,02		
	Pilsden from { Mintern Hill						78177
	Pilsden from { Black Down						79110,7
XLVII.	Charton Common	47 39 18,5	—0,10			47 39 18,5	
	Black Down	27 15 14	—0,21			27 15 16	
	Pilsden	105 5 26	—0,60			105 5 25,5	
		179 59 58,5		0,88	—2,38		
	Charton Common from { Black Down						103345
	Charton Common from { Pilsden						49100,3
XLVIII.	Dumpdon	38 33 22,75	—0,12			38 33 22,25	
	Pilsden	47 32 1,25	—0,14			47 32 1	
	Charton Common	93 54 37,25	—0,36			93 54 36,75	
		180 0 1,25		0,66	+0,59		
	Charton Common from { Dumpdon						49016,3
	Charton Common from { Pilsden						78459,3
XLIX.	Little Haldon	25 8 1,25	—0,45			25 8 1	
	Charton Common	68 12 51,25	—0,48			68 12 51	
	Dumpdon	86 39 8,5	—0,78			86 39 8	
		180 0 1		0,66	+0,34		
	Little Haldon from { Charton Common						136353
	Little Haldon from { Dumpdon						126831

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
I.	Cawsand Beacon	43 14 21,25	—0,57			43 14 20	
	Dumpdon -	35 7 7,25	—0,04			35 7 7	
	Little Haldon -	101 38 33,75	—1,93			101 38 33	
		180 0 2,25		3,12	—0,87		
	Cawsand Beacon from { Dumpdon - -						181334
	Cawsand Beacon from { Little Haldon -						106508
II.	Rippin Tor -	124 59 13	—0,08			124 59 11,75	
	Cawsand Beacon	25 30 39,75	+0,01			25 30 38,75	
	Little Haldon -	29 30 10,5	+0,05			29 30 9,5	
		180 0 3,25		0,69	+2,56		
	Rippin Tor from { Cawsand Beacon -						64020,5
	Rippin Tor from { Little Haldon - -						55988,7
III.	Furland - -	39 24 37,25	—0,26			39 24 37	
	Little Haldon -	84 58 43	—0,44			84 58 42,75	
	Rippin Tor -	55 36 40,5	—0,25			55 36 40,25	
		180 0 0,75		0,96	—0,21		
	Furland from { Little Haldon - -						72776
	Furland from { Rippin Tor - -						87851
III.	Furland -	43 38 4,5	—0,52			43 38 4	
	Rippin Tor -	61 59 59,5	—0,38			61 59 59,25	
	Butterton -	74 21 57,25	—0,44			74 21 56,75	
		180 0 1,25		1,15	+0,1		
	Butterton from { Rippin Tor - -						62951
	Butterton from { Furland - -						80547,8

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
LIV.	Bolt Head	62 56 36,5	— 0,41			62 56 35,25	
	Furland	53 15 35	— 0,38			53 15 34,75	
	Butterton	63 47 50,75	— 0,43			63 47 50	
		180 0 2,25		1,23	+ 1,02		
	Bolt Head from { Furland						81152
	Bolt Head from { Butterton						72479,8
LV.	Maker Heights	45 54 37	— 0,42			45 54 37,5	
	Bolt Head	48 39 24,5	— 0,33			48 39 24,5	
	Butterton	85 25 58	— 0,59			85 25 58	
		179 59 59,5		1,29	— 1,79		
	Maker Heights from { Bolt Head						100591
	Maker Heights from { Butterton						75760,8
LVI.	Maker Heights	112 18 8,75	— 1,09			112 18 8	
	Butterton	35 30 28,75	— 0,17			35 30 29	
	Carraton Hill	32 11 23	— 0,10			32 11 23	
		180 0 0,5		1,36	— 0,86		
	Carraton Hill from { Butterton						131576
	Carraton Hill from { Maker Heights						82600,3
LVII.	Lansallos	64 7 44,25	— 0,44			64 7 44	
	Maker Heights	48 39 54,75	— 0,30			48 39 54,5	
	Carraton Hill	67 12 21,75	— 0,43			67 12 21,5	
		180 0 0,75		1,24	— 0,49		
	Lansallos from { Maker Heights						84631,4
	Lansallos from { Carraton Hill						68929,7

By the latter triangle we get the distance from Lansallos to Carraton Hill 68929,7 feet; which being obtained from the least number of triangles, we shall make use of in the calculations of the sides farther to the westward. The same conclusion, however, is nearly obtained by making the computations pass through the triangles connected with Kit Hill and the station on Bindown.

Distance from Butterton to Maker Heights 75760,8 feet.

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
LVIII.	Kit Hill - -	48 36 46,75	— 0,26			48 36 46,75	
	Butterton -	42 11 38,75	— 0,20			42 11 38,75	
	Maker Heights	89 11 34,5	— 0,75			89 11 34,5	
		180 0 0		1,21	— 1,21		
	Kit Hill from { Butterton - -						100969
	Kit Hill from { Maker Heights - -						67822,3
LIX.	Bindown -	75 9 24,5	— 0,28			75 9 24,25	
	Maker Heights	51 29 22,5	— 0,17			51 29 22,25	
	Kit Hill - -	53 21 13,75	— 0,22			53 21 13,5	
		180 0 0,75		0,70	+ 0,05		
	Bindown from { Maker Heights - -						56294,8
	Bindown from { Kit Hill - - - -						54902,7
LX.	Carraton Hill	91 45 22,5				91 45 23	
	Kit Hill -	50 45 31				50 45 31	
	Bindown - -	37 29 5,75				37 29 6	
		179 59 59,25		0,42	— 1,17		
	Carraton Hill from { Kit Hill - - -						33427
	Carraton Hill from { Bindown - -						42541,4
LXI.	Lansallos -	32 36 43,25				32 36 42,25	
	Bindown - -	119 9 36,25				119 9 35,25	
	Carraton Hill	28 13 43,25				28 13 42,5	
		180 0 2,75		0,33	+ 2,42		
	Lansallos from Bindown - - -						37335,3

By the last triangle we get the distance from Lansallos to Carraton 68931 feet. We shall, however, as before observed, use the distance between those stations as derived from the LVII. triangle.

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
LXII.	Lansallos -	54 57 44	— 0,26			54 57 44	
	Carraton Hill	56 21 17	— 0,27			56 21 17	
	Bodmin Down	68 40 59	— 0,30			68 40 59	
		180 0 0		0,82	— 0,82		
	Bodmin Down from { Carraton Hill - -						60582,7
	Bodmin Down from { Lansallos - - -						61597,1
LXIII.	Hensbarrow Beacon	66 59 23,25	— 0,23			66 59 22,25	
	Bodmin Down	67 59 27,75	— 0,21			67 59 26,75	
	Lansallos -	45 1 11,75	— 0,19			45 1 11	
		180 0 2,75		0,63	+ 2,12		
	Hensbarrow Beacon from Bodmin Down -						47337,2

By this last triangle, the distance from Hensbarrow Beacon to Lansallos is found to be 62044,8 feet, and by the following triangle

LXIV.	Hensbarrow Beacon	42 32 8,5	— 0,20			42 32 8	
	Carraton Hill	37 28 58	— 0,18			37 28 57,5	
	Lansallos -	99 58 55,75	— 0,59			99 58 54,5	
		180 0 2,25		0,99	+ 1,26		
	Hensbarrow Beacon from Carraton Hill -						100416

we get 62044,7 feet for the same distance.

LXV.	Deadman -	56 12 22,75	— 0,25			56 12 22,5	
	Lansallos - -	52 34 3	— 0,24			52 34 2,5	
	Hensbarrow Beacon	71 13 35,25	— 0,35			71 13 35	
		180 0 1		0,82	+ 0,18		
	Deadman from { Lansallos - -						70686,8
	Deadman from { Hensbarrow Beacon -						59284,2

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
LXVI.	St. Agnes' Beacon	34 31 20,25	—0,33			34 31 19,75	
	Hensbarrow Beacon	77 20 29,5	—0,54			77 20 28,75	
	Deadman -	68 8 13	—0,63			68 8 12	
		180 0 2,75		1,32	+1,43		
	St. Agnes' Beacon from { Hensbarrow Beacon -						97084.8
	St. Agnes' Beacon from { Deadman - -						102066
LXVII.	St. Agnes' Beacon	75 51 53,75	—0,40			75 51 53,5	
	Deadman -	25 51 24,75	—0,30			25 51 25,25	
	Karnbonellis -	78 16 41	—0,40			78 16 41,25	
		179 59 59,5		1,06	—1,56		
	Karnbonellis from { Deadman - - -						101084
	Karnbonellis from { St. Agnes' Beacon -						45461,9
LXVIII.	Karnminnis -	32 30 0,25	—0,22			32 30 0,25	
	St. Agnes' Beacon	57 46 31,25	—0,35			57 46 31	
	Karnbonellis -	89 43 29	—0,53			89 43 28,75	
		180 0 0,5		0,77	—0,27		
	Karnminnis from { St. Agnes' Beacon -						84610.6
	Karnminnis from { Karnbonellis - -						71578,3
LXIX.	St. Buryan -	41 43 45,5	—0,03			41 43 45,25	
	Karnbonellis -	26 22 59,25	—0,09			26 22 59,25	
	Karnminnis -	111 53 16	—0,65			111 53 15,5	
		180 0 0,75		0,75	0,0		
	St. Buryan from { Karnbonellis - - -						99786
	St. Buryan from { Karnminnis - - -						47786,7

No. of triangles	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
LXX.	Pertinney -	113 40 15,5				113 40 15	
	Karnminnis -	13 48 18				13 48 18	
	St. Buryan -	52 31 27,5				52 31 27	
		180 0 1		0,16	+0,84		
	Pertinney from { Karnminnis - -						41407,7
	Pertinney from { St. Buryan - -						12450,2
LXXI.	Sennen - -	36 39 18,75				36 39 18,25	
	St. Buryan -	75 36 11,5				75 36 11	
	Pertinney - -	67 44 31				67 44 30,75	
		180 0 1,25		0,08	+1,17		
	Sennen from { St. Buryan - - -						19300,8
	Sennen from { Pertinney - - -						20199,9

The angles in the above series of triangles, are those arising from taking the means of the several observations: and the same rules have been adopted for their corrections, which were laid down in the account of the trigonometrical operation, published in the *Philosophical Transactions* for 1795. The angle at Blackdown in the XLVII. triangle (for the triangles of the present series are numbered in order from those of the former), is considered to be nearly 9″ in defect, and has been augmented for calculation accordingly: the angle at that station was observed under circumstances less favourable, than those which attended the observations made on Pilsden, and Charton Common.

SECTION THIRD.

Heights of the Stations. Terrestrial Refractions.

ART. 1. *Elevations and Depressions.*

At Wingreen.

The ground at Bull Barrow - -	*depressed*	6	3

At Nine Barrow Down.

The ground at Black Down - -	*depr.*	3	29
at Bull Barrow - -	*elevated*	1	25

At Black Down.

The ground at Nine Barrow Down -	*depr.*	13	26
at Charton Common -	*depr.*	15	11
at Mintern Hill - -		0	0
at Bull Barrow - -	*depr.*	1	16
at Pilsden - - -	*depr.*	0	50

At Pilsden Hill.

The ground at Black Down - -	*depr.*	11	0
at Charton Common -	*depr.*	28	39
The horizon of the sea on the 6th of June, at 6 P. M. in a S. E. direction, nearly,	*depr.*	29	23

At Bull Barrow.

The ground at Wingreen - -	*depr.*	4	53
at Mintern - - -	*depr.*	6	5
at Black Down - -	*depr.*	10	39

On Charton Common.

		°	′
The ground at Black Down			
at Pilsden	*elev.*	20	37
at Haldon	*depr.*	3	33

At Dumpdon.

The ground at Pilsden	*depr.*	3	45
at Charton	*depr.*	22	12
The bottom of the Karn, or heap of stones, (nearly on a level with the axis of the telescope) on Cawsand Beacon	*elev.*	4	42

At Haldon.

The ground at Charton	*depr.*	15	59
at Cawsand Beacon	*elev.*	24	3
at Rippin Tor	*elev.*	40	49
at Furland	*depr.*	16	6
The horizon of the sea on the 27th of July, at 6 P. M. in a S. W. direction, nearly,	*depr.*	27	24

On Cawsand Beacon.

The ground at Rippin Tor	*depr.*	17	42
at Haldon	*depr.*	38	57
The lamp at Dumpdon	*depr.*	29	36

N. B. The lamp was about $5\frac{1}{2}$ feet from the ground.

On Rippin Tor.

The ground at Butterton	*depr.*	28	38
at Cawsand Beacon	*elev.*	8	3
at Haldon	*depr.*	49	31

At Furland.

The ground at Haldon	-	-	*elev.*	5 27
at Butterton	-	-	*elev.*	20 15

At Butterton.

The ground at Kit Hill	-	-	*depr.*	10 49
at Carraton	-	-	*depr.*	9 0
at Maker Heights	-	-	*depr.*	41 48
at the Bolt Head	-	-	*depr.*	41 48
at Furland	-	-	*depr.*	32 18
at Rippin Tor	-	-	*elev.*	18 54

On Maker Heights.

The ground at Lansallos	-	-	*depr.*	1 27
at Bindown	-	-	*elev.*	11 32
at Carraton Hill	-	-	*elev.*	27 36
at Kit Hill	-	-	*elev.*	29 45
at Butterton	-	-	*elev.*	30 45
at the Bolt Head	-	-	*depr.*	5 47

At the Bolt Head.

The ground at Maker	-	-	*depr.*	7 42
at Butterton	-	-	*elev.*	31 6

At Kit Hill.

The ground at Butterton	-	-	*depr.*	1 42
at Maker Heights	-	-	*depr.*	37 38
at Bindown	-	-	*depr.*	31 0
at Carraton Hill	-	-	*elev.*	9 38

On Carraton Hill.

The ground at Lansallos	- - -	*depr.* 41	18
at Hensbarrow	- -	*depr.* 13	27
at Maker Heights	- -	*depr.* 39	30
at Bindown	- - -	*depr.* 47	48
at Butterton	- - -	*depr.* 9	48
at Kit Hill	- -	*depr.* 15	19

On Bindown.

The ground at Maker Heights	- -	*depr.* 19	41
at Carraton Hill	- -	*elev.* 41	20
at Lansallos	- -	*depr.* 16	24
at Hensbarrow	- -	*elev.* 7	10
at Kit Hill	- - -	*elev.* 22	51

At Lansallos.

The ground at Carraton Hill	- -	*elev.* 30	18
at Bindown	- - -	*elev.* 10	46
at Kit Hill	- - -	*elev.* 15	27
at Bodmin Down	- -	*elev.* 2	56
at Hensbarrow	- -	*elev.* 23	57
at the Deadman	- -	*depr.* 11	39
at Maker Heights	- -	*depr.* 10	30

On Bodmin Down.

The ground at Hensbarrow	- -	*elev.* 24	3
at Lansallos	- - -	*depr.* 12	9

On Hensbarrow Beacon.

The ground at Carraton	-	-	*depr.* 0 36
at Lansallos	-	-	*depr.* 33 23
at the Deadman	-	-	*depr.* 42 8
at St. Agnes' Beacon		-	*depr.* 21 53
at Bodmin Down	-	-	*depr.* 31 21

At the Deadman.

The ground at Karnbonellis	-	-	*elev.* 7 51
at St. Agnes' Beacon		-	*elev.* 0 19
at Hensbarrow	-	-	*elev.* 33 30
at Lansallos	-	-	*elev.* 1 30

At St. Agnes' Beacon.

The ground at Karnminnis	-	-	*elev.* 2 11
at Karnbonellis	-	-	*elev.* 12 45
at Hensbarrow	-	-	*elev.* 8 8
at the Deadman	-	-	*depr.* 14 15

On Karnbonellis.

The ground at St. Agnes' Beacon		-	*depr.* 19 51
at Karnminnis	-	-	*depr.* 5 51
at St. Buryan	-	-	*depr.* 20 56
at the Deadman	-	-	*depr.* 22 18

On Karnminnis.

The ground at St Buryan	-	-	*depr.* 32 9
at Karnbonellis	-	-	*depr.* 4 30
at St. Agnes' Beacon		-	*depr.* 14 12
at Pertinney Hill	-	-	*depr.* 9 14

At St. Buryan.

The ground at Karnminnis	- -	*elev.* 24′ 32″
at Karnbonellis	- -	*elev.* 6 50

N. B. 6″ must be subtracted from the elevations, and added to the depressions, on account of the error in the parallelism of the line of collimation of the telescope, and the rod attached to its side, upon which the level is hung.

The axis of the telescope was about $5\frac{1}{2}$ feet from the ground at all the above stations.

ART. II. *Terrestrial Refractions.*

Between	Mean Refraction.
Maker and Kit Hill - -	$\frac{1}{7}$ of the contained arc.
Butterdon and Kit Hill -	$\frac{1}{8}$
Bindown and Lansallos -	$\frac{1}{9}$
Nine Barrow Down and Black Down	$\frac{1}{10}$
Maker and Lansallos -	$\frac{1}{10}$
Maker and the Bolt Head -	$\frac{1}{10}$
Carraton Hill and Bindown -	$\frac{1}{11}$
Karnbonellis and St Buryan	$\frac{1}{11}$
Maker and Bindown - -	$\frac{1}{12}$
Hensbarrow and the Deadman	$\frac{1}{13}$
St. Agnes' Beacon and the Deadman	$\frac{1}{12}$
St. Agnes' Beacon and Karnminnis	$\frac{1}{12}$
Dumpdon and Cawsand Beacon	$\frac{1}{13}$
Haldon and Cawsand Beacon	$\frac{1}{13}$
Kit Hill and Bindown -	$\frac{1}{13}$
Carraton Hill and Hensbarrow	$\frac{1}{13}$

Between	Mean Refraction.
Lansallos and the Deadman -	$\frac{1}{15}$ of the contained arc.
Hensbarrow and St. Agnes' Beacon	$\frac{1}{15}$
Karnbonellis and Karnminnis	$\frac{1}{14}$
Furland and Haldon - -	$\frac{1}{15}$
Butterton and Maker -	$\frac{1}{15}$
Butterton and Carraton Hill -	$\frac{1}{15}$
Maker and Carraton Hill -	$\frac{1}{15}$
Karnbonellis and the Deadman	$\frac{1}{15}$
Karnbonellis and St. Agnes' Beacon	$\frac{1}{15}$
Karnminnis and St. Buryan -	$\frac{1}{15}$
Hensbarrow and Bodmin Down	$\frac{1}{15}$
Lansallos and Bodmin -	$\frac{1}{15}$
Butterton and the Bolt Head -	$\frac{1}{15}$
Haldon and Charton Common	$\frac{1}{17}$
Rippin Tor and Cawsand Beacon	$\frac{1}{17}$
Black Down and Bull Barrow	$\frac{1}{18}$
Black Down and Pilsden Hill	$\frac{1}{18}$
Black Down and Charton Common	$\frac{1}{18}$
Lansallos and Hensbarrow -	$\frac{1}{18}$
Rippin Tor and Haldon -	$\frac{1}{19}$
Butterton and Furland -	$\frac{1}{19}$
Butterton and Rippin Tor -	$\frac{1}{21}$
Kit Hill and Carraton -	$\frac{1}{26}$
Pilsden Hill and Charton Common	$\frac{1}{28}$
Wingreen and Bull Barrow -	$\frac{1}{31}$
Lansallos and Carraton Hill -	$\frac{1}{34}$
Haldon and the Horizon of the Sea	$\frac{1}{11}$
Pilsden Hill and the Horizon of the Sea	$\frac{1}{11}$

The mean refractions were found by the following rules.

1. Reduce the elevations, or depressions, to the place of the axis of the telescope at each station, by adding, or subtracting, as the case may require, the angle at the place of observation, subtended by the vertical height between the object, whose elevation or depression was observed, and the axis of the telescope when at that station.*

2. Then, if both are depressions, subtract their sum from the contained arc, and half the remainder is the mean refraction.

3. If one is a depression and the other an elevation, take their difference. Then, if the depression is greater than the elevation, subtract the difference from the contained arc, and half the remainder is the mean refraction. But if the elevation is greatest, add the difference to the contained arc, and half the sum is the mean refraction.

ART. 111. *Table containing the Heights of the Stations.*

Stations.	Heights.
Black Down - -	817 feet.
Charton Common -	582
Little Haldon - -	818
Rippin Tor - -	1549
Furland - -	589

* For example. At the station on Hensbarrow, the ground at Bodmin Down was depressed 31′ 27″: the distance of those stations is 47337 feet; and the axis of the telescope was 5½ feet above the ground: therefore, as 47337 : *radius* :: 5½ feet : *tang.* 24″ the angle subtended by 5½ feet at that distance; which, taken from 31′ 27″, gives 31′ 3″ for the depression of the place of the axis, instead of the ground. Again, at Bodmin Down, the ground at Hensbarrow was elevated 23′ 57″, to which adding 24″, we have 24′ 21″ for the elevation of the place of the axis.

Stations.	Heights.
Butterton	1203 feet.
Maker Heights	402
Bull Barrow	927
Mintern Hill	891
Pilsden Hill	934
Dumpdon	879
Cawsand Beacon	1792
Bolt Head	480
Kit Hill	1067
Bindown	658
Carraton Hill	1208
Lansallos	514
Bodmin Down	649
Hensbarrow Beacon	1026
The Deadman	379
St. Agnes' Beacon	599
Karnbonellis	822
Karnminnis	805
St. Buryan	415

ART. IV. *Remarks, &c. on the foregoing Table.*

The height of the ground at the station on Maker Heights, 402 feet, was determined by levelling down to low-water mark, near the passage house, below Mount Edgcumbe, on April 15, 1796. This, however, had been done several years before, by some officers of the Royal Engineers, who found it to be 401 feet. The height of the station near Dunnose, in the Isle of Wight, was also found by levelling; of which an account is given in the Philosophical Transactions for 1795. It therefore

may be considered as the least exceptionable mode of procedure, to deduce the intermediate heights from both those stations; for which purpose, the following comparison was made, exhibiting the height of the station on Charton Common, both ways.

	Feet.
Height of Nine Barrow Down (Phil. Trans. 1795, p. 582)	642
of Black Down - - -	825
of Charton Common, *deduced from the height of* Dunnose - - - -	597
Height of Butterton - - - - -	1201
of Rippin Tor - - - -	1545
of Furland - - - -	585
of Haldon - - - - -	811
of Charton Common, *deduced from the height of* Maker - - -	568
from that of Dunnose	597
difference	29

Those are the heights resulting directly from the observations. Now, supposing the difference, or the errors, to arise from the mean refractions, and those errors to be nearly the same between every two stations, we shall obtain the corrected heights in the following manner:

				Feet.	
Nine Barrow Down	642	−	4	= 638	
Black Down	825	−	8	= 817	
Charton Common	597	−	15	= 582	
Butterton -	1201	+	2	= 1203	as in the table.
Rippin Tor	1545	+	4	= 1549	
Haldon -	811	+	7	= 818	
Charton Common	568	+	14	= 582	

From those corrected heights, the others to the northward have been deduced. The heights to the westward of Butterton were determined from that of Maker. A mean of two or three results, by using $\frac{1}{15}$ of the contained arcs for refraction, is taken for the height of the station on Mintern Hill.

We subjoin the following elevations and depressions, for the use of those who may wish to examine the tables of heights and refractions, in the Philosophical Transactions for 1795. And here it is to be noted, that the axis of the telescope was always about $5\frac{1}{2}$ feet from the ground, unless the contrary is specified.

At Hanger Hill.

		°	′
The ground at St. Ann's Hill	*depr.*	4	36
at Banstead	*elev.*	10	39

At St. Ann's Hill.

The ground at Bagshot Heath	*elev.*	11	23	Instrument on the half scaffold: the axis of the telescope $20\frac{1}{2}$ feet high.
at Banstead	*elev.*	10	2	
at Hanger Hill	*depr.*	6	13	
The top of the flagstaff near Hampton Poor House	*depr.*	12	54	

N. B. The flagstaff was about 41 feet high.

Near Hampton Poor House.

The ground at St. Ann's Hill	*elev.*	8	17	Instrument on the whole scaffold: the axis about $36\frac{1}{2}$ feet high.

At Banstead.

		°	′	
The ground at Leith Hill	*elev.*	17	29	
at Shooter's Hill	*depr.*	11	7	
at St. Ann's Hill	*depr.*	22	9	On the half scaffold: the axis 20½ feet high.
at Hanger Hill	*depr.*	22	35	
The top of the flagstaff at Botley Hill - -	*elev.*	18	0	The staff about 29 feet high.

At Leith Hill.

The top of the flagstaff at Banstead -	*depr.*	25	57	The staff about 27½ feet high.
of the flagst. at Botley Hill	*depr.*	8	46	
The ground at Hind Head	*depr.*	8	28	
at Crowborough Beacon	*depr.*	13	48	
at Ditchling Beacon	*depr.*	12	34	
at Chanctonbury Ring	*depr.*	13	10	
The top of Severndroog Castle	*depr.*	22	9	

N. B. The axis of the telescope when at Shooter's Hill, was about 29½ feet lower than the top of the Castle.

At Shooter's Hill.

The ground at Leith Hill	*elev.*	2	35
at Banstead	*elev.*	0	15

On Bagshot Heath.

The ground at Hind Head	*elev.*	10	37
at St. Ann's Hill	*depr.*	12	30

At Hind Head.

The ground at Leith Hill	*depr.*	2	59
at Chanctonbury Ring	*depr.*	11	11

The ground at Rook's Hill	*depr.*	14	51	
at Butser Hill	*depr.*	5	54	
at Bagshot Heath	*depr.*	23	12	
at Highclere	*depr.*	10	42	

On Rook's Hill.

The ground at Hind Head	*elev.*	3	9	
at Chanctonbury Ring	*depr.*	1	35	
at Bow Hill -	*depr.*	1	5	
at Portsdown -	*depr.*	16	22	

At Butser Hill.

The ground at Highclere	*depr.*	9	29	
at Hind Head -	*depr.*	4	44	
at Motteston Down	*depr.*	15	27	

At Chanctonbury Ring.

The ground at Rook's Hill	*depr.*	10	46	
at Hind Head	*depr.*	4	20	
at Leith Hill	*depr.*	1	13	
at Beachy Head	*depr.*	16	27	On the half scaffold: the axis 20½ feet high.

At Dunnose.

The ground at Nine Barrow Down - -	*depr.*	15	37	
at Dean Hill	*depr.*	17	24	

On Ditchling Beacon.

The ground at Leith Hill	*depr.*	4	36

On Fairlight Down.

The ground at Beachy Head	*depr.*	7	45
at Brightling Windmill	*depr.*	0	49

The ground at the Windmill is about 4 feet higher than the axis of the telescope when at Brightling.

On Brightling Down.

The ground at Fairlight Down	*depr.*	7	56
at Beachy Head -	*depr.*	8	44
at Crowborough Beacon	*elev.*	3	54

At Crowborough Beacon.

The ground at Leith Hill	*depr.*	4	8
at Brightling Windmill	*depr.*	12	21
at Botley Hill -	*depr.*	8	5

At Beachy Head.

The ground at Fairlight Down	*depr.*	5	17
at Brightling Windmill	*depr.*	1	48
at Chanctonbury Ring	*depr.*	5	6

At Dean Hill.

The ground at Highclere	*elev.*	0	46
at Beacon Hill	*elev.*	4	47
at Wingreen	*elev.*	5	5
at Dunnose	*depr.*	7	56

At Beacon Hill.

The ground at Highclere	*depr.*	0	′	″ 15
at Wingreen	*depr.*	0		34
at Dean Hill	*depr.*	13		13

At Highclere.

The ground at Hind Head	*depr.*	10		42
at Butser Hill	*depr.*	9		26
at Dean Hill	*depr.*	18		12
at Beacon Hill	*depr.*	13		15

On Nine Barrow Down.

The ground at Wingreen	*depr.*	1		20
at Dunnose	*depr.*	10		8

At Wingreen.

The ground at Beacon Hill	*depr.*	15		30
at Nine Barrow Down	*depr.*	17		40
at Dean Hill -	*depr.*	20		19

SECTION FOURTH.

Containing the secondary Triangles, in which two Angles only have been observed. The first three intersected Places are intended for the small Instrument, on Account of their commanding Situations.

ART. 1. *Triangles.*

Distance from Pilsden Hill to Charton Common 49016,3 Feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
		° ′ ″		Feet.
157	Pilsden -	44 6 35	Golden Cape	29848
	Charton Common	36 59 6		34533
	Golden Cape			

Distance from Rippin Tor to Cawsand Beacon 64020,5 feet.

158	Rippin Tor -	88 2 28	Great Haldon -	54789
	Cawsand Beacon	41 22 57		82829
	Great Haldon			

Distance from the Bolt Head to Maker Heights 100591 feet.

159	Bolt Head -	29 15 10	Hemmerdon Ball	82239
	Maker Heights -	54 20 9		49464
	Hemmerdon Ball			

Distance from Bull Barrow to Wingreen 69058 feet.

160	Bull Barrow -	109 12 12	Noil Windmill	63692
	Wingreen -	33 45 11		108255
	Noil Windmill			

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
161	Bull Barrow -	° ′ ″ 22 4 38	Noil Steeple -	88420
	Wingreen -	111 10 59		35641
	Noil Steeple			
162	Bull Barrow -	18 16 15	H. Trinity Steeple,	63275
	Wingreen -	65 39 45	Shaftesbury	21772
	Holy Trinity Steeple, Shaftesbury			
163	Bull Barrow -	15 45 15	St. Rumbold's Steeple, Shaftesbury	56778
	Wingreen -	46 55 34		21104
	St. Rumbold's Steeple, Shaftesbury			
164	Bull Barrow -	129 15 18	Maypowder Steeple	24199
	Wingreen -	12 31 19		86426
	Maypowder Steeple			
165	Bull Barrow -	44 25 52	Stourhead House	94319
	Wingreen -	88 31 14		66050
	Stourhead House			

Distance from Bull Barrow to Nine Barrow Down 106213 feet.

166	Bull Barrow -	32 25 49	Mr. Frampton's	56980
	Nine Barrow Down	27 44 1	Obelisk -	65662
	Mr. Frampton's Obelisk			

Bull Barrow from Mintern, or Revel's Hill, 42653,4 feet.

167	Bull Barrow -	97 43 51	Mere Steeple -	88095
	Mintern -	58 1 14		102912
	Mere Steeple			

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
168	Bull Barrow Mintern *Mrs. Thornhill's Obelisk*	° ′ ″ 68 44 5 47 19 3	Mrs. Thornhill's Obelisk	34902 44245
169	Bull Barrow Mintern *Odcombe Steeple*	20 37 56 148 59 47	Odcombe Steeple	94589 56700
170	Bull Barrow D Mintern *Milborne-port Steeple*	52 41 35 77 1 36	Milborne-port Steeple	54038 44107
171	Bull Barrow D Mintern *Lord Poulett's, Warren House*	7 39 0 132 19 30	Warren House	8829 49035

Distance from Black Down to Pilsden 79110,7 feet.

172	Black Down Pilsden *Portland Light-house*	143 32 28 16 12 4	Light-House	63749 135775
173	Black Down Pilsden *Naval-Signal-staff on Puncknoll*	32 55 8 18 35 5	Signal-staff at Puncknoll	25615 59266
174	Black Down Pilsden *House in Lambert's Castle*	9 2 48 62 47 53	Lambert's Castle	74048 13091

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
175	Black Down -	° ′ ″ 26 6 41	Lyme Cobb -	90349
	Pilsden - -	92 54 15		39815
	Lyme Cobb			

Distance from Pilsden to Mintern 78177 feet.

176	Pilsden - -	64 47 55	Glastonbury Tor	127174
	Mintern -	78 12 22		117551
	Glastonbury Tor			

Distance from Pilsden to Charton Common 49016,3 feet.

177	Pilsden - -	40 30 48	Bridport Beacon	44332
	Charton Common	62 0 1		32616
	Bridport Beacon, a Sea-mark			
178	Pilsden - -	15 44 0	Barn on Sidmouth Hill - -	39824
	Charton Common	45 18 13		15191
	Barn on the high land near Sidmouth			

Distance from Dumpdon to Pilsden 78459 feet.

179	Dumpdon -	50 52 11	Signal-staff on Whitlands -	50832
	Pilsden - -	40 22 12		60876
	Naval-Signal-staff on Whitlands			
180	Dumpdon -	93 52 54	Catherstone Lodge	64521
	Pilsden - -	87 51 16		104901
	Catherstone Lodge, Quantock Hills			

Distance from Charton Common to Dumpdon 58012,4 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
		° ′ ″		Feet.
181	Charton Common Dumpdon - *Lord Lisburne's Obelisk on Haldon*	61 11 28 91 51 33	Lord Lisburne's Obelisk	127936 112161

Distance from Dumpdon to Cawsand Beacon 181334 feet.

182	Dumpdon - Cawsand Beacon *Sir J. de la Pole's Flagstaff, near Sbute House*	128 45 59 13 59 24	Sir J. de la Pole's Flagstaff	72435 233619
183	Dumpdon - Cawsand Beacon *Honiton Steeple*	64 18 8 4 0 39	Honiton Steeple	13650 175852
184	Dumpdon - Cawsand Beacon *St. Mary Ottery Steeple*	34 20 21 12 27 16	St. Mary Ottery Steeple -	53653 140335

Distance from Little Haldon to Dumpdon 126831 feet.

185	Dumpdon - Little Haldon - *Funnel on Sir R. Palk's Tower, Haldon*	17 20 53 63 7 37	Sir R. Palk's Tower	114716 38347

Distance from Cawsand Beacon to Little Haldon 106508 feet.

186	Cawsand Beacon D Little Haldon - *North Bovey Steeple*	7 9 50 10 38 19	North Bovey Steeple - -	64313 43444

Distance from Little Haldon to Rippin Tor 55988,7 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
187	Little Haldon -	° ′ ″ 34 8 22	Eastern Karn, near Moreton Hampstead -	52099
	Rippin Tor -	66 14 23		31944
	Eastern Karn, or heap of stones, on the high ground near Moreton Hampstead			
188	Little Haldon -	37 24 5	Western Karn near Moreton Hampstead -	54751
	Rippin Tor -	69 24 30		35525
	Western Karn, near Moreton Hampstead			
189	Little Haldon -	154 35 29	Naval-Signal-staff, West Down Beacon - -	46268
	Rippin Tor -	11 28 37		99715
	Naval-Signal-staff at West Down Beacon			
190	Little Haldon -	5 43 59	Summer House	55462
	Rippin Tor -	81 44 20		5598
	Mr. Woodley's Summer House			
191	Little Haldon	99 46 2	Signal-staff on Berry Head	62040
	Rippin Tor -	42 35 24		90345
	Naval-Signal-staff, Berry Head, Torbay			
192	Little Haldon	91 52 49	Brixen Steeple	66070
	Rippin Tor -	48 37 47		87993
	Brixen Steeple			

No.	Triangles.	Observed angles.			Distances of the stations from the intersected objects.		
		°	′	″			Feet.
193	Little Haldon	67	8	45	Ipplepen Steeple		42675
	Rippin Tor -	44	56	5			55677
	Ipplepen Steeple						
194	Little Haldon	20	40	42	Three Barrow Tor		81460
	Rippin Tor -	125	6	32			35163
	Three Barrow Tor, Dartmoor						

Distance from Furland to Little Haldon 72776 feet.

195	Furland - -	71	56	38	Brent Tor -		68727
	Little Haldon	51	46	15			83180
	Brent Tor						

Distance from Butterton to Rippin Tor 62951 feet.

196	Butterton -	17	4	21	Chudleigh Steeple		97802
	Rippin Tor -	136	27	46			41471
	Chudleigh Steeple						

Distance from Butterton to Furland 80547,8 feet.

197	Butterton -	3	37	11	Naval-Signal-staff at Coleton		87814
	Furland - -	140	5	47			8593
	Naval-Signal-Staff at Coleton, near Froward Point						
198	Butterton -	39	15	6	Naval-Signal-staff, Start Point		89129
	Furland - -	78	26	47			57561
	Naval-Signal-staff, Start Point						

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
199	Butterton -	61 55 7	Marlborough Steeple - -	64099
	Furland - -	48 18 25		75736
	Marlborough Steeple			
200	Butterton -	63 40 32	Naval-Signal-staff on the Bolt Head	72632
	Furland - -	53 24 17		81084
	Naval-Signal-staff, near the Bolt Head			

Distance from Butterton to Maker Heights 75760,8 feet.

201	Butterton -	18 0 46	Mewstone -	62728
	Maker - -	50 17 40		25213
	Higbest Part of the Mewstone			
202	Butterton -	6 11 21	Cupola of the Royal Hospital	68709
	Maker Heights	44 49 37		10508
	Cupola of the Royal Hospital, Plymouth			
203	Butterton -	8 58 35	St. John's Steeple	85401
	Maker Heights	122 49 11		15856
	St. John's Steeple			
204	Butterton -	19 46 39	Saltash Steeple	73708
	Maker Heights	75 36 25		23749
	Saltash Steeple			
205	Butterton -	5 36 20	Penlee Beacon	76972
	Maker Heights	96 23 55		7566
	Penlee Beacon			

Distance from Butterton to Kit Hill 1c0969 feet.

No.	Triangles.	Observed angles.			Distances of the stations from the intersected objects.		Feet.
		°	′	″			
206	Butterton -	39	1	33	Plymstock Steeple		51259
	Kit Hill - -	27	49	38			69143
	Plymstock Steeple						
207	Butterton -	48	3	55	Statten Barn		58906
	Kit Hill - -	35	25	31			75599
	Statten Barn						
208	Butterton -	41	56	57	Mount Batton		62087
	Kit Hill - -	37	8	33			68738
	Mount Batton						
209	Butterton -	39	56	31	Flagstaff, Plymouth Garrison		59673
	Kit Hill - -	34	45	12			67207
	Flagstaff in Plymouth Garrison						
210	Butterton -	37	21	59	New Church Steeple - -		58899
	Kit Hill - -	33	0	38			63058
	New Church Steeple at Plymouth						
211	Butterton -	37	45	52	Old Church Steeple - -		59524
	Kit Hill - -	34	3	52			65081
	Old Church Steeple at Plymouth						
212	Butterton -	37	5	33	Governor's House, Plymouth Dock		66558
	Kit Hill - -	39	58	36			62479
	West Chimney of the Governor's House, Plymouth Dock						

No.	Triangles.	Observed angles.			Distances of the stations from the intersected objects.		Feet.
213	Butterton -	° 37	′ 6	″ 53	Flagstaff on Mount Wise -		67374
	Kit Hill - -	40	42	48			62327
	Flagstaff in the Fort on Mount Wise						
214	Butterton -	35	14	20	The Chapel, Plymouth Dock		68658
	Kit Hill - -	41	25	1			59874
	Steeple of the Chapel, Plymouth Dock						
215	Butterton -	41	40	8	Flagstaff in St. Nicholas' Island		63970
	Kit Hill - -	38	38	52			68097
	Flagstaff in St. Nicholas' Island						
216	Butterton -	38	40	39	Obelisk at Crimhill Passage		69376
	Kit Hill - -	42	48	20			63803
	Obelisk at Crimbill Passage						
217	Butterton -	40	29	28	Mount Edgcumbe House -		69096
	Kit Hill - -	42	49	8			66012
	East Pinnacle on Mount Edgcumbe House						
218	Butterton -	41	54	7	Maker Tower		72001
	Kit Hill - -	45	25	27			67507
	Flagstaff on Maker Tower						
219	Butterton -	41	58	45	Naval-Signal-staff near Maker Tower		72207
	Kit Hill - -	45	35	55			67490
	Naval-Signal-staff, near Maker Tower						

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
220	Butterton -	° ′ ″ 12 40 29	Chestow Steeple	138522
	Kit Hill - -	138 21 13		45738
	Chestow Steeple			

Distance from Butterton to Carraton Hill 131576 feet.

221	Butterton -	40 34 1	Stonehouse Steeple	58310
	Carraton Hill -	23 29 2		95162
	Stonehouse Steeple			
222	Butterton -	60 48 52	Obelisk at Puslinch	38700
	Carraton Hill -	16 41 16		117659
	Obelisk at Puslinch			
223	Butterton -	41 2 54	Rame Head -	84846
	Carraton Hill -	39 30 40		87594
	Rame Head			

Distance from Kit Hill to Maker Heights 67822,3 feet.

224	Kit Hill - -	116 24 26	Brent Tor -	43421
	Maker Heights -	24 3 10		95419
	Brent Tor, near Lidford			
225	Kit Hill - -	11 30 56	Block House -	57984
	Maker Heights -	46 26 51		15972
	Flag-staff of the Block House, near Dock			
226	Kit Hill - -	4 3 42	Rame Steeple -	74547
	Maker Heights -	141 4 28		8403
	Rame Steeple			

Distance from Carraton Hill to Maker Heights 82600,3 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
227	Carraton Hill -	° ′ ″ 7 28 15	Dock-yard Chapel	78468
	Maker Heights -	64 48 50		11274
	Steeple of the Chapel in the Yard, Plymouth Dock			
228	Carraton Hill -	7 34 6	Windmill at Plymouth Dock	79778
	Maker Heights -	71 29 35		11080
	Windmill at Plymouth Dock			
229	Carraton Hill -	7 31 7	Statten Battery	97488
	Maker Heights -	133 32 55		17199
	Battery on Statten Heights			

Distance from Kit Hill to Carraton Hill 38427 feet.

230	Kit Hill - -	105 0 39	St. Stephen's Steeple - -	44659
	Carraton Hill -	48 47 30		62830
	St. Stephen's Steeple			
231	Kit Hill - -	29 11 14	St. Ive Steeple	25390
	Carraton Hill -	47 42 54		16786
	St. Ive Steeple			
232	Kit Hill - -	42 31 4	Callington Steeple	7582
	Carraton -	10 20 54		28386
	Callington Steeple			
233	Kit Hill - -	25 20 11	LinkinhornSteeple	19621
	Carraton Hill -	28 8 55		17798
	Linkinborn Steeple			

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
234 D	Kit Hill - -	121 48 23	St. Dominic Steeple	7776
	Carraton Hill -	9 59 38		38097
	St. Dominic Steeple			
235 D	Kit Hill - -	60 22 24	South Petherwin Steeple -	39475
	Carraton Hill -	67 55 47		37027
	South Petherwin Steeple			
236	Kit Hill - -	19 31 2	South Hill Steeple	15493
	Carraton Hill -	15 22 32		19522
	South Hill Steeple			
237	Kit Hill - -	108 14 2	House at Empercombe -	64348
	Carraton Hill -	48 46 11		81266
	Lord Mount Edgcumbe's House, at Empercombe			
238	Kit Hill - -	59 59 7	Sea-mark on the Hoe - -	66387
	Carraton Hill -	42 59 48		87011
	Northern Sea-mark on the Hoe			

Distance from Kit Hill to Bindown 54902,7 feet.

239	Kit Hill - -	89 56 21	St. Cleer Steeple	42931
	Bindown -	51 25 10		35256
	St. Cleer Steeple			

Distance from Carraton Hill to Bindown 42541,4 feet.

No.	Triangles.	Observed angles.			Distances of the stations from the intersected objects.		Feet.
		°	′	″			
240	Carraton Hill -	130	14	2	Brownwilly -	{	48221
	Bindown - -	26	32	44		{	82371
	The highest part of Brownwilly						
241	Carraton Hill -	138	42	49	Cheese Rings -	{	9773
	Bindown - -	7	21	53		{	50800
	Cheese Rings						
242	Carraton Hill -	18	2	57	Liskeard Steeple	{	21739
	Bindown - -	17	6	59		{	22885
	Liskeard Steeple						
243	Carraton Hill -	18	6	21	Duloe Steeple -	{	43403
	Bindown - -	84	32	47		{	13550
	Duloe Steeple						
244	Carraton Hill -	9	16	26	Menheniot Steeple	{	21502
	Bindown - -	14	32	34		{	13806
	Menheniot Steeple						
245	Carraton Hill -	48	17	44	Landrake Steeple	{	47177
	Bindown - -	75	46	11		{	33376
	Landrake Steeple						
246	Carraton Hill -	22	51	23	Signal-staff at Nea-	{	36203
	Bindown - -	129	59	13	land -	{	71413
	Naval-Signal-staff at Nealand, near Polparrow						

Distance from Lansallos to Carraton Hill 68929,7 feet.

No.	Triangles.	Observed angles.			Distances of the stations from the intersected objects.		Feet.
		°	′	″			
247	Carraton Hill -	25	5	53	Boconnock Steeple		46079
	Lansallos - -	35	41	57			33495
	Boconnock Steeple						
248	Carraton Hill -	24	4	10	Obelisk at Bocon-		50139
	Lansallos - -	41	27	47	nock -		30886
	Obelisk at Boconnock, (Lord Camelford's)						
249	Carraton Hill -	41	29	10	Roach Rock -		99410
	Lansallos - -	94	48	32			66086
	Roach Rock						
250	Carraton Hill -	42	1	28	Roach Steeple		100214
	Lansallos - -	94	41	58			67314
	Roach Steeple						

Distance from Lansallos to Hensbarrow Beacon 62044,8 feet.

251	Lansallos -	21	34	34	Helmen Tor -		48412
	Hensbarrow Beacon	46	16	45			24633
	Helmen Tor						
252	Lansallos -	37	8	29	Summer House		61105
	Hensbarrow Beacon	70	7	42			39231
	Mr. Tremaine's Summer House						
253	Lansallos -	45	34	10	Gorran Steeple		66624
	Hensbarrow Beacon	72	8	29			50008
	Gorran Steeple						

No.	Triangles,	Observed angles.	Distances of the stations from the intersected objects.	
		° ′ ″		Feet.
254	Lansallos - -	52 48 25	Naval-Signal-staff	71136
	Hensbarrow Beacon	71 28 51	at the Deadman	59696
	Naval-Signal-staff on the Deadman			
255	Lansallos - -	51 21 9	Gwineas Rocks, off	57977
	Hensbarrow Beacon	60 17 27	Mevagissy	52133
	Gwineas Rocks			

Distance from Bodmin Down to Hensbarrow Beacon 47337,2 feet.

256	Bodmin Down	97 21 30	Hendellion Steeple	44851
	Hensbarrow Beacon	39 57 45		69255
	Hendellion Steeple			
257	Bodmin Down	48 38 46	The high Stone on	39924
	Hensbarrow Beacon	55 1 58	St. Braeg Down	36571
	The high Stone on St. Braeg Down			
258	Bodmin Down	13 28 31	St. Dennis Steeple	56722
	Hensbarrow Beacon	120 37 11		15359
	St. Dennis Steeple			
259	Bodmin Down	64 55 8	Lansallos Steeple	61011
D	Hensbarrow Beacon	68 45 47		59285
	Lansallos Steeple			

Deadman Head from Lansallos 70686,8 feet.

260	Deadman -	12 51 38	St. Veep Steeple	67986
D	Lansallos - -	73 45 53		15761
	St. Veep Steeple			

Lansallos from Bodmin Down 61597,1 feet.

No.	Triangles.	Observed angles.			Distances of the stations from the intersected objects.		Feet.
		°	′	″			
261	Lansallos - -	26	19	35	Lanlivery Steeple		39552
D	Bodmin Down	33	51	19			31486
	Lanlivery Steeple						

Hensbarrow Beacon from Deadman Head 59284,2 feet.

262	Hensbarrow Beacon	30	50	7	Gerrans Steeple		83901
D	Deadman -	106	31	21			44858
	Gerrans Steeple						
263	Hensbarrow Beacon	13	56	6	St. Michael Carhayes Steeple		48309
D	Deadman -	43	10	53			17001
	St. Michael Carhayes Steeple						
264	Hensbarrow Beacon	31	22	22	St. Kivern Steeple		136676
	Deadman -	128	53	52			91426
	St. Kivern Steeple						
265	Hensbarrow Beacon	29	6	51	Signal-staff at Black Head		146770
	Deadman -	133	59	31			99260
	Naval-Signal-staff at Black Head						
266	Hensbarrow Beacon	62	46	29	Fowey Windmill		45036
	Deadman -	45	59	37			55677
	Windmill near Fowey						
267	Hensbarrow Beacon	56	10	33	Menabilly House		35221
	Deadman -	86	24	22			49300
	Menabilly House						

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
268	Hensbarrow Beacon	60° 28′ 23″	Old Tower at Polruan	47561
	Deadman -	49 6 10		54749
	Old Tower at Polruan			
269	Hensbarrow Beacon	39 52 0	Signal-staff, St. Anthony's Head	98759
	Deadman -	116 42 13		56717
	Naval-Signal-staff at St. Anthony's Head			

Distance from Hensbarrow Beacon to St. Agnes' Beacon 97084.8 feet.

270	Hensbarrow Beacon	31 37 12	St. Columb Minor Steeple -	53942
D	St. Agnes' Beacon	28 56 16		58448
	St. Columb Minor Steeple			
271	Hensbarrow Beacon	11 43 0	Peranzabulo Steeple - -	73829
D	St. Agnes' Beacon	31 9 89		28975
	Peranzabulo Steeple			
272	Hensbarrow Beacon	57 24 41	St. Eval Steeple	56011
	St. Agnes' Beacon	85 11 34		81884
	St. Eval Steeple			
273	Hensbarrow Beacon	15 2 26	Cubert Steeple	69141
	St. Agnes' Beacon	30 37 20		35224
	Cubert Steeple			
274	Hensbarrow Beacon	41 44 14	Pendennis Castle	101687
	St. Agnes' Beacon	72 36 24		70988
	Flagstaff in Pendennis Castle			

No.	Triangles.	Observed angles.			Distances of the stations from the intersected objects.		Feet.
		°	′	″			
275	Hensbarrow Beacon	42	11	25	Windmill near St. Mawes		87286
	St. Agnes' Beacon	61	3	38			66985
	Windmill near St. Mawes						
	Distance from St. Agnes' Beacon to Karnminnis 84610,6 feet.						
276	St. Agnes' Beacon	49	20	11	Karnbre Castle		81435
	Karnminnis	20	23	49			68417
	Karnbre Castle						
277	St. Agnes' Beacon	55	59	58	Cupola in Redruth		26903
	Karnminnis	17	46	35			73054
	Cupola of the Market House in Redruth						
278	St. Agnes' Beacon	30	57	7	Camborn Steeple		39427
	Karnminnis	21	45	40			54696
	Camborn Steeple						
279	St. Agnes' Beacon	31	12	56	Illugan Steeple		28718
	Karnminnis	10	49	6			65490
	Illugan Steeple						
280	St. Agnes' Beacon	40	52	42	St. Paul Steeple		110564
	Karnminnis	117	47	27			81794
	St. Paul Steeple						
281	St. Agnes' Beacon	20	40	33	Lord de Dunstanville's House		80339
	Karnminnis	10	47	12			57237
	Lord de Dunstanville's House						

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
		° ′ ″		Feet.
282 D	St. Agnes' Beacon	21 40 24	Gwinear Steeple	62144
	Karnminnis	40 30 44		35330
	Gwinear Steeple			
283	St. Agnes' Beacon	53 24 45	Mr. Kneil's Obelisk	73889
	Karnminnis	88 37 42		59846
	Mr. Kneil's Obelisk, near St. Ives			
284	St. Agnes' Beacon	141 53 34	Cow and Calf Rocks	94650
	Karnminnis	20 9 34		169450
	Highest of the Rocks called the Cow and Calf			

Distance from St. Agnes' Beacon to Karnbonellis 45461,9 feet.

285	St. Agnes' Beacon	94 43 5	St. Erme Steeple	44668
	Karnbonellis	42 10 34		66303
	St. Erme Steeple			
286	St. Agnes' Beacon	98 13 52	St. Allen Steeple	36816
	Karnbonellis	35 41 11		62462
	St. Allen Steeple			
287	St. Agnes' Beacon	44 12 31	Ludgvan Steeple	87573
	Karnbonellis	105 49 41		63469
	Ludgvan Steeple			

Distance from Karnminnis to Karnbonellis 71578,8 feet.

288	Karnminnis	41 26 59	Lizard Windmill	104413
	Karnbonellis	93 31 22		69440
	Windmill near the Lizard			

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
289	Karnminnis	° ′ ″ 40 7 0	Grade Steeple	110762
	Karnbonellis	100 25 15		72566
	Grade Steeple			
290	Karnminnis	38 32 27	Ruan Major Steeple - -	102248
	Karnbonellis	97 80 19		64256
	Ruan Major Steeple			
291	Karnminnis	39 52 32	St. Hilary Steeple	33808
	Karnbonellis	25 24 25		50519
	St. Hilary Steeple			
292	Karnminnis	10 0 52	Castle Dennis	69233
	Karnbonellis	74 13 53		15749
	Castle Dennis (Mr. Rogers's Tower)			

Distance from Karnbonellis to St. Buryan 99786 feet.

293	Karnbonellis	9 32 41	Madern Steeple	80908
	St. Buryan	33 51 25		24081
	Madern Steeple			
294	Karnbonellis	60 38 57	Perranuthno Steeple - -	38552
D	St. Buryan	49 18 46		44315
	Perranuthno Steeple			
295	Karnbonellis	76 57 1	Girnhove Steeple	46355
D	St. Buryan	50 25 48		58583
	Girnhove Steeple			
296	Karnbonellis	60 25 48	Signal-staff -	66344
	St. Buryan	40 48 1		88458
	Naval-Signal-staff, Park Loughs			

Distance from Pertinney to Karnminnis 41407,7 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
297	Pertinney	116 12 46	St. Buryan Steeple	12751
	Karnminnis	13 40 7		48411
	St. Buryan Steeple			

Distance from St. Buryan to Pertinney 12450,2 feet.

298	St. Buryan	23 28 57	Chapel Karnbury	10728
	Pertinney	58 34 54		5009
	Chapel Karnbury			
299	St. Buryan	75 36 7	Signal-staff, St. Leven's Point	20094
	Pertinney	67 31 4		19169
	Naval-Signal-staff, St. Leven's Point			
300	St. Buryan	69 21 10	Sennen Steeple	17475
	Pertinney	68 58 0		17520
	Sennen Steeple			

Distance from Sennen to Pertinney 20199,9 feet.

301	Sennen	106 43 44	Stone near the Land's End	2791
	Pertinney	7 15 12		21173
	Stone near the Land's End			
302	Sennen	126 1 11	Longship's Light-house	10717
	Pertinney	18 6 39		27883
	Longship's Light-house			

The above triangles, and those which follow in this section, are numbered in order from the secondary series, given in the Philosophical Transactions for 1795.

ART. II. *Triangles for ascertaining the Distances of the Eddystone Light-house, from the Flagstaff of Plymouth Garrison, and the Rame-head.*

The ball on the lantern of the Light-house was observed from the stations on Butterton, Kit Hill, and Carraton Hill; and as much uncertainty has heretofore existed, with respect to a knowledge of its true distance from any point in the neighbourhood of Plymouth, observations were made on various arcs of the circle of the instrument, at the two first stations.

The triangles are the following.

Distance from Butterton to Kit Hill 100969 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
303	Butterton Kit Hill *Eddystone Light-house*	66° 46′ 21″ 64 27 46	Eddystone Light-house	121159 123899

Distance from Butterton to Carraton Hill 131576 feet.

304	Butterton Carraton Hill *Eddystone Light-house*	60 5 31 55 52 41	Eddystone Light-house	121158 126863

With the distance of the Eddystone Light-house from Kit Hill, and also that of the Flagstaff in Plymouth garrison from the same station, we find the distance from the Light-house to the Flagstaff = 73061 feet;* the observed angle being 29° 42′ 34″: and, computing with the *data* obtained from the last triangle, and the 223d,

* On referring to the late Mr. Smeaton's Narrative of the Building of the Eddystone Light-house, it will be found, that, from a trigonometrical process, founded on two bases measured on the Hoe, among other deductions, he concluded the distance between the above objects was 73464 feet; being 403 greater than the distance found by the above computation.

with the observed angle at Carraton Hill = 16° 22′ 1″, we get 49435 feet for the distance of the Eddystone Light-house from the building on Rame-head. It may be proper to observe, that the Eddystone Light-house is nearer to the Rame-head than to any other point on the coast.

ART. III. *Triangles for ascertaining the Situations of the Lizard Light-houses; and the Lizard Point.*

Distance from Karnbonellis to Pertinney 101474 feet.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
305	Karnbonellis -	78° 49′ 28″	Eastern Light-house - -	81323
	Pertinney - -	42 56 51		117097
	Eastern Light-house			
306	Karnbonellis -	78 40 5	Western Light-house - -	81348
	Pertinney - -	43 0 53		116921
	Western Light-house			
307	Karnbonellis -	78 8 57	Signal-staff -	79635
	Pertinney - -	42 28 45		115408
	Naval-Signal-staff			

Distance from Karnbonellis to St. Buryan 99786 feet.

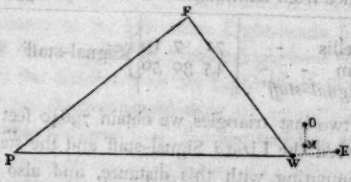
308	Karnbonellis -	71 7 19	Signal-staff -	79645
	St. Buryan - -	45 30 56		105873
	Naval-Signal-staff			

From the two last triangles we obtain 79640 feet for the mean distance between the Lizard Signal-staff and the station on Karnbonellis. Computing with this distance, and also that from the Western Light-house to the same station, with the observed angle 0° 31′ 8″, we get 1857 feet for the distance between those objects.

For the purpose of ascertaining the situation of the Lizard Point, two angles in the following triangle were observed with a sextant, *viz.*

Naval-Signal-staff	-	77° 4′
Western Light-house	-	60 50
Lizard Point		

These, with the computed distance from the Signal-staff to the Light-house, give the distance of the Lizard Point from the $\left\{\begin{matrix}\text{Signal-staff} & 2419 \\ \text{Light-house} & 2700\end{matrix}\right\}$ feet. Hence, the distance of the point from the station on Karnbonellis is 81085 feet, the angle at that station, between the Lizard Point and Western Light-house, being 1° 53′ 47″. With respect to the means by which the situation of the spot, on which Mr. BRADLEY erected his observatory in 1769, may hereafter be determined, it will be readily understood from the following diagram; where E is the Eastern Light-house, W the Western Light-house, F the Signal-staff, P the Lizard Point, and O the place of the Observatory. The distance between the spot O, and M,* the place where his meridian mark was fixed, we measured and found = 800 feet; M being 24 feet north of the line joining the centres of the Light-houses.



* The person spoken of in Sect. 1. Art. 3. as having the care of the Light-houses, pointed out this spot.

ART. IV. *Triangles for finding the Distances of the Day-Mark, St. Agnes' Light-house, and other Objects in the Scilly Isles, from particular Stations in the West of Cornwall.*

Observations made at Karnminnis.

Between	°	′	″	Mean.
The station at St. Buryan and the Day-Mark	89	3	22¾ 22¾ 23¾	″ 23
At St. Buryan.				
Karnminnis and the Day-Mark -	129	52	22 22¼	22
Pertinney and St. Agnes' Light-house -	83	59	51¾ 50	51
Flagstaff of the fort in St. Mary's and Karnminnis - - - -	134	39	45¾ 45	45½
Windmill in St. Mary's and Pertinney -	84	23	53½ 53	53¼
At Pertinney.				
St. Agnes' Light-house and Karnminnis	92	6	20 21¼ 21½ 23½	21¾
Day-Mark and Karnminnis - -	148	11	8½ 10¼	9¼
Flagstaff in St. Mary's and St. Buryan	93	47	18	
Windmill in St. Mary's and St. Buryan	92	26	33	
At Sennen.				
Day-Mark and Pertinney - -	145	20	8½ 10	9¼
St. Agnes' Light-house and Pertinney -	152	43	24 24½	24¼

From those observations, result the following triangles, when the necessary corrections are applied for reducing the observed angles to those formed by the chords, *viz.*

Distance from Karnminnis to St. Buryan 47786,7 feet.

No.	Triangles.	Observed angles cor.	Distances of the stations from the intersected objects.	Feet.
		° ′ ″		
309	Karnminnis -	39 3 24	Day-Mark -	190985
	St. Buryan - -	129 52 19		156796
	Day-Mark			

Distance from Karnminnis to Pertinney 41407,7 feet.

310	Karnminnis -	25 15 8	Day-Mark -	190989
	Pertinney - -	148 11 5		154551
	Day-Mark			

Distance from Sennen to Pertinney 20199,9 feet.

311	Sennen - -	143 20 7	Day-Mark -	137526
	Pertinney - -	30 24 7		154568
	Day-Mark			
312	Sennen - -	152 48 20	St. Agnes' Light-	164010
	Pertinney - -	24 21 55	house -	182199
	St. Agnes' or the Scilly Light-house			

Distance from St. Buryan to Pertinney 12450,2 feet.

313	St. Buryan - -	83 59 51	St. Agnes' Light-	183096
	Pertinney - -	92 6 22	house - -	182215
	St. Agnes' Light-house			

No.	Triangles.	Observed angles cor.	Distances of the stations from the intersected objects.	
				Feet.
314	St. Buryan - -	83° 24′ 53″	Windmill in St. Mary's -	172183
	Pertinney - -	92 26 33		171203
	Windmill in St. Mary's			
315	St. Buryan - -	82 8 18	Flagstaff in St. Mary's -	174890
	Pertinney - -	93 47 18		173626
	Flagstaff of the fort in St. Mary's			

The distance from the Day-Mark to Karnminnis, as obtained from the 309th triangle, is 190985 feet, and by the 310th, 190989 feet, which differs only 4 feet from the former; and by the 310th and 311th triangles, the difference of the distances from the same object, to the station on Pertinney, is 17 feet; which, allowing for the shortness of the bases, must be considered as trifling. We may presume, therefore, that had not the Day-Mark been seen from Karnminnis, but from Sennen and Pertinney alone, the observations from which the angles of the 311th triangle are derived, would have afforded the means of computing the distance with sufficient precision. In like manner the 312th and 313th triangles seem to prove, that the observations made to St. Agnes' Light-house were sufficiently accurate, as there is a difference only of 16 feet between the distances of the Light-house from Pertinney. The ball on the top of the Light-house was the object always observed; and the Day-Mark being pyramidical, we had the means of making the observations at the different stations to the same point of this building.

ART. V. *Of the Distances of the Objects in the Scilly Isles, (intersected from the Stations in the West of Cornwall) from Sennen Steeple; the Stone near the Land's End; and the Longship's Light-house.*

As the observations made to the Day Mark, and St. Agnes' Light-house, may be supposed sufficiently accurate; and the ball on the top of the Longship's Light-house was also observed under favourable circumstances, it will be proper to apply the corrections to the horizontal angles, in order to obtain those formed by the chords. Taking, therefore, Pertinney as the angular point, and computing with the following *data, viz.*

Station on Pertinney from - { Day-Mark - - = 154551; St. Agnes' Light-house = 182207; Longship's Light-house = 27883 } Feet. And

the angle at Pertinney, augmented for calculation, between the Longship's Light-house and { the Day-Mark = 12° 17′ 30″; St. Agnes' Light-house = 6 15 25 } We get the distance of

the Longship's Light-house from - { the Day-Mark - = 127446 feet = 24,14; St. Agnes' Light-house = 154519 feet = 29,06 } Miles.

Calculating also, with the distances of the two other objects in the Scilly Isles, and likewise those of Sennen Steeple, and the Stone near the Land's End from Pertinney, with the included angles at the same station, we get

		Feet.	Miles.
Sennen Steeple from -	Day Mark - -	= 139521	= 26,43
	St. Agnes' Light-house	= 166255	= 31,49
	Flagstaff in St. Mary's	= 157912	= 29,95
	Windmill in St. Mary's	= 155299	= 29,41
Stone near the Land's End from -	Day Mark - -	= 135343	= 25,63
	St. Agnes' Light-house	= 162100	= 30,7
	Flagstaff in St. Mary's	= 153744	= 29,11
	Windmill in St. Mary's	= 151138	= 28,63

Of the Scilly Isles, Menawthen is the nearest to the Land's End, being about $1\frac{2}{10}$ miles eastward of the Day-Mark; and the cluster of rocks, called the Bishop and his Clerks, the most remote, being $3\frac{1}{3}$ miles west of St. Agnes' Light-house. Combining, therefore, the above particulars with those distances, we may conclude, that the nearest part of the Scilly Isles is about 24,7 miles from the Land's End, and the farthest nearly 34.

PART SECOND.

SECTION I.

Account of a Trigonometrical Survey carried on in Kent, in the Years 1795, and 1796, with the small circular Instrument.

ARTICLE 1. *Particulars respecting the Instrument.*

The instrument used in this survey was announced in the Philosophical Transactions for 1795, p. 590. It was made by Mr. RAMSDEN; and is about half the size of his large theodolite, or circular instrument, with which we take the horizontal angles, but nearly similar to it in all its parts; consequently a very brief description will be sufficient.

The most material variations in the construction are,

1. The levelling or feet screws. These are below that horizontal movement which serves to direct the lower telescope to any particular object. By this position of the screws, the horizontal circle being once made level, the whole instrument may be moved round without disturbing its horizontality; the levelling screws remaining stationary during that operation,

which cannot be done in the large instrument, because the screws are carried round with it.

2. The diameter of the horizontal circle being only half that of the larger one, it follows, that the space between any two dots on the limb, gives double the number of minutes that are contained in the same space on the greater circle: on this account, each revolution in the micrometer screw in the microscope answers to 2′; and the circle on the microscopic micrometer being divided into 60 parts, each division becomes equal to 2″, but for the conveniency of notation, they are numbered at every 5th, with 10, 20, &c. to 50, the 60th being marked 1, to denote 1′: the number of seconds then commencing as before, the whole revolution becomes 2′. The revolutions are counted by means of notches on one side of the field in the microscope, in the same manner as in those of the large instrument.

3. This instrument not being intended for determining the direction of the meridian, a vertical semicircle for directing the telescope to the pole star became unnecessary; yet some apparatus was required, whereby small elevations or depressions from the horizon might be ascertained with a tolerable degree of precision. For this purpose, a moveable index, of about four inches long, is made to turn on the horizontal axis of the upper telescope, and so constructed, that by means of a finger screw, it can be fixed firmly in any position. The lower end of this index is furnished with a steel micrometer screw, having a circle on its head, divided into 100 parts, for shewing the fractional parts of a revolution, while other divisions, on a chamfered edge of the index which marks the fractional parts, give the number of revolutions made by the micrometer screw.

The method of finding the value of a revolution of the micrometer head in parts of a degree, &c. was as follows:

A rod, 14 or 16 feet long, was placed horizontally about three quarters of a mile off, and the angle subtended by its ends measured with the instrument in the usual way: the rod was then set up perpendicular at the same place, and the cross wires in the telescope directed to one of its extremities: the telescope was then moved in the vertical plane, by means of the micrometer screw, till the cross wires coincided with the other extremity. In this manner, by counting the number of revolutions, &c. necessary to move the telescope from one position to the other, an angle was measured vertically with the micrometer screw, equal to the former horizontal angle. From repeated trials, the value of a revolution was found equal to 10′ 27″.

This instrument, on account of its portable size, may very readily be taken to the tops of steeples, towers, &c. and is, therefore, extremely well adapted to the uses for which it was intended.

ART. II. *Situations of the Stations on which Observations were made with the small circular Instrument, in the Summer of the Year* 1795.

Folkstone Turnpike, the station made use of by General Roy in 1787.

Hawkinge, about three quarters of a mile from Folkstone Turnpike. This station was chosen for the purpose of having a view of the Belvidere in Waldershare Park, which cannot be seen from the station of 1787.

Dover Castle.

Paddlesworth; about 400 feet from the station of 1787. This new spot was selected, because Hardres Steeple is not visible from the old station.

Waldersbare; on the Belvidere in the Earl of Guilford's Park.

On *Ringswold Steeple.*

On a sand hill near the sea shore, between Deal and Ramsgate: this station is denominated *Shore.*

Near *Mount Pleasant House*, Isle of Thanet.

On a rising ground near *Wingham.*

On *Chislet* Steeple.

In *Beverley Park*, near Canterbury.

On Upper *Hardres* Steeple.

ART. III. *Triangles for determining the Distances of the Stations.*

As the station on the Keep of Dover Castle, in 1787, was directly over the steps of the Turret, a new point was chosen about $6\frac{1}{2}$ feet from the former, where the instrument could stand conveniently: this new point is about 2,8 feet farther from Folkstone Turnpike, and 1 foot farther from Paddlesworth, than the point marking the old station.

From General Roy's Account of the Trigonometrical Survey in 1787, we have

Dover Castle from Folkstone Turnpike	31554,6	feet.
from Paddlesworth	42561,2	

Now, augmenting those distances in the proportion of 141747 to 141753 (see Phil. Trans. Vol. LXXX, p. 595, and the Vol.

for 1795, p. 508), we get 31556, and 42563 feet; to which adding 2,8, and 1, respectively, we have

The new point on Dover Castle from Folkstone Turnpike - - - - - 31558,8
from Paddlesworth 42564 } feet.

In order to obtain the distance between Waldershare and Dover Castle from those new sides, or distances, the three angles of the following triangle were very carefully taken.

1 { Dover Castle - 3° 49′ 16″ 3° 49′ 15″
Folkstone Turnpike 36 6 31 36 6 30
Hawkinge - 140 4 16 140 4 15 } for computation.

The third angles of the two next triangles were not observed:

2 { Hawkinge - - - 44° 23′ 30″
Dover Castle - - 73 53 44
Waldershare - - - 61 42 46

3 { Dover Castle - - - 62 24 7
Paddlesworth (the station of 1787) 32 36 9
Waldershare - - - 84 59 44

By the two first triangles, Dover Castle from Waldershare 23019,4
From the latter - - 23021,5 } 23020,5 mean distance.

And *Hawkinge* from { Dover Castle 28976
Waldershare 31616

N. B. The angles at the stations, or objects, denoted in *italics*, are supplemental, or were not observed. And it is also to be remarked, that whenever Paddlesworth is mentioned hereafter, the *new station* is to be understood.

No.	Names of stations.	Observed angles.			Distances.		Feet.
		°	′	″			
4	Waldershare	85	2	25	Paddlesw. from	Dover	42239
	Paddlesworth	32	53	10		Waldershare	37460
	Dover -	62	4	25			
5	Waldershare	57	1	15	Hardres	Waldershare	48548
	Paddlesworth	69	21	59		Paddlesworth	39035
	Hardres	53	36	46			
6	Dover	66	46	45	Ringswold	Dover	23745
	Waldershare	57	57	24		Waldershare	25743
	Ringswold	55	15	51			
		180	0	0			
7	Waldershare	45	43	8	Shore	Waldershare	42755
	Ringswold	97	38	32		Ringswold	30883
	Shore -	36	38	20			
8	Mount Pleasant	40	53	17	Mt. Pleasant	Shore	30635
	Shore -	111	8	27		Waldershare	60920
	Waldershare	27	58	16			
9	Mount Pleasant	38	32	17	Chislet	Mount Pleasant	30062
	Chislet -	79	25	36—35		Wingham	21206
	Wingham	62	2	8			
		180	0	1			
10	Hardres	52	46	14	Hardres from Wingham		39322
	Wingham	69	29	1			
	Waldershare	57	44	55			
11	Wingham	50	4	0	Beverley Park	Wingham	33320
	Beverley Park	75	0	0		Hardres	31215
	Hardres	54	56	4—0			
		180	0	4			

ART. IV. *Secondary Triangles.*

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
				Feet.
12	Paddlesworth -	38 28 36	Windmill -	37283
	Waldershare -	70 22 24		24628
	Barham Windmill			
13	Dover - -	51 40 11	St. Radigund's Abbey - -	16196
	Waldershare	44 23 40		18160
	St. Radigund's Abbey			
14	Dover - -	75 15 45	Hougham Steeple	16614
	Waldershare -	40 31 40		24726
	Hougham Steeple			
15	Dover - -	32 41 51	Gunston Steeple	9111
	Waldershare -	17 46 31		16123
	Gunston Steeple			
16	Dover - -	88 19 36	St. Margaret's Steeple -	14444
	Waldershare -	32 34 23		26817
	St. Margaret's Steeple			
17	Hawkinge -	84 50 30	Elham Windmill	8335
	Waldershare -	15 3 14		31963
	Elham Windmill			
18	Dover - -	39 48 39	South Foreland Light-house	12081
	Ringswold -	28 8 7		16403
	South Foreland Light-house			
19	Waldershare -	17 10 7	Upper Deal Windmill - -	28870
	Ringswold -	102 11 7		8718
	Upper Deal Windmill			

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
20	Waldershare -	° ′ ″ 22 20 10	Upper Deal Chapel	30160
	Ringswold -	100 38 27		11663
	Upper Deal Chapel			
21	Waldershare -	19 1 31	Lower Deal Windmill -	31226
	Ringswold -	110 21 19		10857
	Lower Deal Windmill			
22	Waldershare -	19 28 27	Deal Castle	34689
	Ringswold -	121 2 45		13498
	Deal Castle			
23	Waldershare -	42 26 26	Norbourn Windmill -	22102
	Ringswold -	57 41 19		17648
	Norbourn Windmill			
24	Waldershare -	9 19 40	Watch-house	31817
	Ringswold -	135 28 8		7238
	Watch-house near the Sea shore			
25	Waldershare -	29 45 47	Sandown Castle	38185
	Ringswold -	111 20 13		20351
	Sandown Castle			
26	Waldershare -	12 29 13	Walmer Steeple	29491
	Ringswold -	115 33 51		7069
	Walmer Steeple			
27	Waldershare -	15 35 53	Ripple Steeple	24209
	Ringswold -	69 33 23		6947
	Ripple Steeple			

No.	Triangles.	Observed angles.			Distances of the stations from the intersected objects.		Feet.
		°	′	″			
28	Waldershare	20	45	23	Waldershare Steeple	-	5656
	Ringswold -	5	35	50			20552
	Waldershare Steeple						
29	Waldershare	16	23	49	Eastry Steeple		25766
	Shore - -	21	57	46			19448
	Eastry Steeple						
30	Waldershare	35	10	6	Ash Steeple -		35750
	Shore - -	56	41	26			24639
	Ash Steeple						
31	Waldershare	28	29	39	Minster Steeple		55782
	Shore - -	103	15	30			27341
	Minster Steeple						
32	Waldershare -	5	43	2	Woard Steeple		33548
	Shore - -	19	37	24			9951
	Woard Steeple						
33	Waldershare -	13	35	31	Sandwich Steeple		38505
	Shore - -	59	30	36			10501
	Sandwich, highest Steeple						
34	Ringswold -	24	46	49	Mongeham Steeple		11379
	Shore - -	13	3	56			21098
	Mongeham Steeple						
35	Ringswold -	35	9	0	Norbourn Steeple		15450
	Shore - -	25	59	2			20303
	Norbourn Steeple						

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
36	Ringswold -	38 7 44	Woodnessborough Steeple -	32320
	Shore - -	77 48 16		18071
	Woodnessborough Steeple			
37	Shore - -	41 10 35	Ramsgate Windmill -	22695
	Mount Pleasant	47 47 27		20173
	Ramsgate Windmill			
38	Shore - -	36 26 58	St. Lawrence Steeple -	25064
	Mount Pleasant	54 52 36		18205
	St. Lawrence Steeple			
39	Waldershare -	32 2 55	Wingham Steeple	35214
	Mount Pleasant	31 1 14		36259
	Wingham Steeple			
40	Waldershare -	31 12 40	Goodneston Steeple -	24841
	Mount Pleasant	17 58 32		41711
	Goodneston Steeple			
41	Mount Pleasant	77 19 0	Birchington Steeple -	11500
	Chislet - -	22 10 4		29735
	Birchington Steeple			
42	Mount Pleasant	19 36 3	St. Nicholas Steeple - -	16690
	Chislet - -	21 19 41		15394
	St. Nicholas Steeple			
43	Mount Pleasant	16 56 56	Stormouth Steeple -	21519
	Chislet - -	38 29 54		11366
	Stormouth Steeple			

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
44	Mount Pleasant	° ′ ″ 22 14 40	Reculver Windmill	30556
	Chislet - -	81 14 59		11703
	Reculver Windmill			
45	Mount Pleasant	69 57 57	South Reculver	31012
	Wingham -	51 54 46		37017
	South Reculver			
46	Mount Pleasant	50 51 41	Hearne Windmill	42669
	Wingham -	78 50 42		38732
	Hearne Windmill			
47	Wingham -	102 34 17	Littlebourn Steeple -	7752
	Waldershare -	11 3 35		39442
	Littlebourn Steeple			
48	Wingham -	58 30 34	Blean Steeple	39329
	Chislet - -	88 52 9		33544
	Blean Steeple			
49	Wingham -	59 11 7	Wickham Steeple	8824
	Chislet - -	24 25 37		18326
	Wickham Steeple			
50	Wingham -	72 3 26	Ickham Steeple	8001
	Chislet - -	22 6 13		20228
	Ickham Steeple			
51	Wingham -	47 35 34	Bridge Windmill	23584
	Beverley Park -	44 59 50		24628
	Bridge Windmill			

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
		° ′ ″		Feet.
52	Wingham -	33 27 20	Nackington Steeple -	31688
	Beverley Park -	68 29 54		18776
	Nackington Steeple			
53	Wingham -	80 53 7	Chillendon Windmill -	15031
	Hardres - -	21 53 16		39811
	Chillendon Windmill			
54	Wingham -	122 1 10	Preston Steeple	7220
	Hardres - -	8 3 28		43572
	Preston Steeple			
55	Wingham -	30 49 24	Shottenden Windmill -	67736
	Hardres - -	118 30 8		39494
	Shottenden Windmill			
56	Hardres - -	11 35 23	St. Martin's Windmill -	22943
	Beverley Park -	27 48 16		9881
	St. Martin's Windmill			
57	Hardres - -	12 11 37	Harbledown Steeple - -	25289
	Beverley Park -	39 25 30		8411
	Harbledown Steeple			
58	Hardres - -	17 29 59	Sturry Steeple	31691
	Beverley Park -	84 3 53		9581
	Sturry Steeple			
59	Waldershare -	24 29 21	Canterbury Cathedral -	54827
	Hardres - -	105 36 14		23597
	Canterbury Cathedral			

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
		° ′ ″		Feet.
60	Hardres Paddlesworth *West-Stone-Street* *Windmill*	40 45 34 27 23 18	West-Stone-Street Windmill	19347 27458
61	Hardres Paddlesworth *Stelling Windmill*	31 0 20 15 3 20	Stelling Windmill	14081 27924

ART. V. *Triangles carried over another Part of Kent in* 1795: *with Remarks.*

On account of the high woody lands to the westward of Hardres and Paddlesworth, the triangles could not be extended in that direction, and therefore the following may be considered as a detached part of the Survey this year.

The Stations were,

Westwell Down,
Wye Down,
Brabourn Down,
Allington, or *Aldington Knoll*, the station of 1787.

Allington Knoll from Tenterden, according to General Roy's account, is 61775.3 feet, which increased in the proportion of 141747 to 141753 becomes 61778 feet. The centre of the top of Tenterden Steeple is about 4 or 4½ feet farther from Allington Knoll than the point marking the station in 1787; therefore the distance of the centre from Allington Knoll will be 61782 feet, which is used in the following computations; because, as a flagstaff of moderate height

cannot be easily distinguished among the pinnacles at any considerable distance, it was thought it might be sufficiently accurate for the present purpose, to intersect the steeple itself.

Triangles for determining the Distances of the Stations.

No.	Stations.	Observed angles.	Distances.		Feet.
62	Allington Knoll	61° 37′ 46″	Westwell D. from	Tenterden	58629
	Westwell Down	68 0 16		Allington K.	51316
	Tenterden	50 21 58			
63	Allington Knoll	34 37 37	Wye Down	Allington K.	37363
	Westwell Down	45 54 19		Westwell D.	29562
	Wye Down	99 28 5—4			
		180 0 1			
64	Allington Knoll	96 15 23	Wye Down	Allington	37360
	Wye Down	54 19 24		Tenterden	75603
	Tenterden	29 25 13			
65	Wye Down	45 8 41	Westwell D. from Wye D.		29566
	Westwell Down	113 54 35			
	Tenterden	20 56 44			
66	Allington Knoll	116 49 40	Brabourn D.	Allington K.	26437
	Brabourn Down	45 25 31		Tenterden	77897
	Tenterden	17 44 49			
67	Allington Knoll	55 11 54	Brabourn D.	Westwell D.	42233
	Brabourn Down	93 52 23		Allington K.	26435
	Westwell Down	30 55 43			

ART. VI. *Secondary Triangles.*

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
68	Wye Down -	° ′ ″ 42 20 58	Ashford Steeple	23922
	Westwell Down	53 35 53		20023
	Ashford Steeple			
69	Wye Down -	86 44 28	Brook Steeple	7983
	Westwell Down	15 18 43		30181
	Brook Steeple			
70	Wye Down -	60 6 18	Willsborough Steeple -	21881
	Westwell Down	45 28 29		26607
	Willsborough Steeple			
71	Wye Down -	58 2 28	Willsborough Windmill	19916
	Westwell Down	41 37 0		25448
	Willsborough Windmill			
72	Wye Down -	58 20 46	KingsnorthSteeple	32498
	Westwell Down	65 40 7		30360
	Kingsnorth Steeple			
73	Wye Down -	52 13 44	Shadoxhurst Steeple - -	41118
	Westwell Down	85 50 2		34966
	Shadoxhurst Steeple			
74	Wye Down -	26 38 18	Kennington Steeple - -	16989
	Westwell Down	27 54 54		16271
	Kennington Steeple			
75	Wye Down -	62 23 7	Great Chart Steeple - -	34029
	Allington Knoll	54 24 4		37083
	Great Chart Steeple			

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
76	Wye Down Allington Knoll *Westwell Steeple*	° ′ ″ 96 45 26 33 49 30	Westwell Steeple	27384 48851
77	Westwell Down Allington Knoll *Pluckley Steeple*	97 22 43 20 53 1	Pluckley Steeple	20768 57778
78	Westwell Down Allington Knoll *Eastwell Steeple*	37 55 0 7 17 0	Eastwell Steeple	9168 44441
79	Westwell Down Allington Knoll *Charing Steeple*	146 22 23 5 24 0	Charing Steeple	10211 60085
80	Westwell Down Allington Knoll *Allington Steeple*	3 15 4 57 34 51	Allington Steeple	49609 3333
81	Brabourn Steeple Allington Knoll *Lymne Steeple*	34 50 49 75 59 12	Lymne Steeple	27443 16161
82	Brabourn Down Allington Knoll *Mersham Steeple*	33 12 51 45 9 19	Mersham Steeple	19136 14784
83	Brabourn Down Allington Knoll *Monks Horton Steeple*	67 22 25 28 46 14	Monks Horton Steeple	10657 24405

The bearings, and distances of the stations and intersected objects, together with their latitudes and longitudes, are given in the following Section.

SECTION II.

Operations in 1796, *with the small circular Instrument.*

ART. I. *Situations of the Stations.*

Lydd
Allington Knoll
High Nook
Fairlight Down
Goudhurst
Tenterden
} Stations in the Survey of 1787.

Westwell Down Station, used in 1795. See Art. v. Section I. Part Second.

Silver Hill, near Robertsbridge. The station is 22 yards S.W. of the Windmill.

Boughton Malherb Steeple.

ART. II. *Triangles for finding the Distances of the Stations.*

From the 5th Article in the last Section, we get the distance from Westwell Down to the *new* station on Tenterden Steeple = 58629,4 feet. This used in the following triangle,

		°	′	″
84	Boughton Malherb	81	55	9
	Westwell Down	63	44	8
	Tenterden –	34	20	48

gives the distance from Boughton Malherb to Westwell Down 38409 feet. Also in the following triangle, using 54376,9 feet for the distance from Tenterden to Goudhurst,

		°	′	″
85	Goudhurst –	52	5	44
	Boughton Malherb	53	54	20
	Tenterden –	75	59	56

we get 33404,5 feet for the distance between the same stations: hence the mean, 33406,8 feet, may be taken for the true distance between Boughton Malherb and Westwell Down. From this latter triangle also, we obtain the distance from Boughton Malherb to Tenterden 53097,6 feet.

No.	Triangles.	Observed angles.			Distances.	
		°	′	″		Feet.
86	Goudhurst	65	29	7		
	Silver Hill	70	32	26	Silver Hill from Goudhurst	40043,1
	Tenterden	43	58	27		

Fairlight Down from Tenterden 71637,7 feet.

87	Fairlight Down	46	34	5		
	Silver Hill	82	25	8	Silver Hill from Fairlight D.	56174,2
	Tenterden	51	0	47		

By the two last triangles, we get 52472,4 and 52481,4 feet for the distances of Tenterden from Silver Hill; the mean of which, 52476,9, we shall hereafter use in determining the distances of the objects, intersected from those stations.

The distance of Goudhurst from Tenterden, and that of Tenterden from Fairlight Down, are derived from those given by General Roy, in the Philosophical Transactions, Vol. LXXX. augmented in the proportion of 141747 to 141753. The distances also, hereafter made use of, between Lydd, and the stations on Fairlight Down, Tenterden Steeple, Allington Knoll, and High Nook, together with that of High Nook from Allington Knoll, are obtained by increasing the distances, found in the same work, in the above proportion. It is proper to

remark, that it has not been thought necessary to reduce the distance between the station on Westwell Down, and the new station on Tenterden Steeple, to that between the former, and the old point at Tenterden, from the trifling difference of those distances.

During the operation of this year, the instrument was also taken to the following stations, *viz.*

Bidenden Steeple,
Hartridge,
Warehorn Steeple,
Stone Crouch,
Iden Steeple.

To determine the distances between these objects, and the stations from whence they were observed, we have the following triangles.

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
88	Goudhurst -	18 16 4	Bidenden Steeple	41100
	Tenterden -	40 0 12		20040
	Bidenden Steeple			
89	Goudhurst -	27 21 34	Hartridge	19134
	Tenterden -	13 14 13		38404
	Hartridge			
90	Allington Knoll	44 16 25	Stone Crouch	51569
	Lydd - -	78 7 50		37627
	Stone Crouch			
91	Allington Knoll	15 46 51	Warehorn -	28100
	Stone Crouch	17 18 22		25690
	Warehorn			

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
92	Tenterden -	° ′ ″ 28 55 46	Iden Steeple -	38239
	Fairlight Down -	20 42 7		45483
	Iden Steeple			

ART. III. *Secondary Triangles.*

93	Goudhurst -	59 47 4	Ulcomb Steeple	56184
	Tenterden -	61 44 12		55123
	Ulcomb Steeple			
94	Goudhurst -	65 36 50	Sutton Windmill	48610
	Tenterden -	52 18 42		56009
	Sutton Windmill			
95	Goudhurst -	70 48 44	Chart Sutton Steeple - -	46338
	Tenterden -	48 11 12		58717
	Chart Sutton Steeple			
96	Goudhurst -	91 32 50	Linton Steeple	41690
	Tenterden -	36 54 6		69407
	Linton Steeple			
97	Goudhurst -	49 11 14	Headcorn Windmill -	40621
	Tenterden -	47 51 2		41468
	Headcorn Windmill			
98	Goudhurst -	29 8 0	Cranbrook Steeple	18239
	Hartridge	70 10 0		9439
	Cranbrook Steeple			
99	Tenterden -	94 50 38	Benenden Steeple	24799
	Boughton Malherb	24 7 11		60471
	Benenden Steeple			

No.	Triangles.	Observed angles.			Distances of the stations from the intersected objects.		Feet.
		°	′	″			
100	Bidenden -	87	0	0	Staplehurst Steeple	Bidenden	25514
	Goudhurst -	38	47	0		Goudhurst	26555
	Staplehurst Steeple						
101	Bidenden -	33	30	0	Marden Steeple	Bidenden	40015
	Goudhurst -	70	42	33		Goudhurst	23899
	Marden Steeple						
102	Boughton Malherb	14	39	40	Frittenden Steeple	Boughton Malherb	36203
	Goudhurst -	17	10	0		Goudhurst	31405
	Frittenden Steeple						
103	Tenterden -	20	46	0	Brasses Windmill	Tenterden	51527
	Silver Hill -	76	45	52		Silver Hill	18768
	Brasses Windmill						
104	Tenterden -	11	2	0	Hawkhurst Steeple	Tenterden	44028
	Silver Hill -	42	17	30		Silver Hill	12522
	Hawkhurst Steeple						
105	Silver Hill -	72	5	37	Sandhurst Steeple	Silver Hill	16448
	Fairlight Down	17	1	25		Fairlight Down	53460
	Sandhurst Steeple						
106	Silver Hill -	58	27	19	Whittersham Steeple -	Silver Hill	50861
	Fairlight Down	55	42	10		Fairlight Down	52469
	Whittersham Steeple						
107	Silver Hill -	38	49	4	Peasemarsh Steeple -	Silver Hill	49016
	Fairlight Down	59	39	38		Fairlight Down	35602
	Peasemarsh Steeple						

No.	Triangles.	Observed angles.			Distances of the stations from the intersected objects.		Feet.
		°	′	″			
108	Silver Hill -	82	8	4	Rolvenden Steeple	{	38028
	Fairlight Down	36	28	0		{	63380
	Rolvenden Steeple						
109	Silver Hill -	42	30	35	Beckley Steeple	{	33419
	Fairlight Down	35	36	7		{	38790
	Beckley Steeple						
110	Allington Knoll	46	3	7	New Church Steeple - -	{	13967
	High Nook -	36	41	43		{	16828
	New Church Steeple						
111	Allington Knoll	52	3	53	Ivy Church Steeple	{	28621
	High Nook -	76	5	26		{	23256
	Ivy Church Steeple						
112	Allington Knoll	27	21	0	St. Mary's Steeple	{	23939
	High Nook -	80	3	0		{	11165
	St. Mary's Steeple						
113	Tenterden -	34	33	5	Playden Steeple	{	40204
	Lydd - -	34	35	48		{	40158
	Playden Steeple						
114	Iden - -	21	57	0	Winchelsea Steeple - -	{	21224
	Fairlight Down	17	5	40		{	26990
	Winchelsea Steeple						
115	Winchelsea -	48	6	0	Brede Steeple	{	26373
	Fairlight Down	67	26	0		{	21755
	Brede Steeple						

No.	Triangles.	Observed angles.			Distances of the stations from the intersected objects.		Feet.
116	Brede Steeple -	° 56	′ 0	″ 0	Icklesham Steeple	{	19091
	Fairlight Down	55	1	0		{	19813
	Icklesham Steeple						
117	Stone Crouch -	55	9	34	Woodchurch Steeple -	{	28098
	Allington Knoll	82	59	15		{	42857
	Woodchurch Steeple						
118	Stone Crouch -	41	36	38	Old Romney Steeple -	{	31037
	Allington Knoll	85	59	89		{	85070
	Old Romney Steeple						
119	Stone Crouch -	41	54	7	New Romney Steeple -	{	40957
	Allington Knoll	52	11	38		{	34544
	New Romney Steeple						
120	Stone Crouch -	40	47	11	Brookland Steeple	{	15919
	Allington Knoll	14	44	21		{	40872
	Brookland Steeple						
121	Stone Crouch -	20	16	5	Orleston Steeple	{	33421
	Allington Knoll	29	46	58		{	23308
	Orleston Steeple						
122	Stone Crouch -	67	14	56	East Guilford Steeple -	{	15782
	Lydd - -	24	46	59		{	34721
	East Guilford Steeple						
123	Stone Crouch -	58	4	1	Snargate Steeple	{	17900
	Lydd - -	28	2	7		{	30443
	Snargate Steeple						

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	Feet.
124	Stone Crouch -	° ′ ″ 25 37 0	Snave Steeple -	26667
	Warehorn Steeple	81 34 0		11629
	Snave Steeple			
125	Stone Crouch -	9 11 12	Appledore Steeple	11016
	Warehorn -	6 46 0		14925
	Appledore Steeple			
126	Warehorn -	91 6 0	Brenzet Steeple	16476
D	Allington Knoll	30 5 41		32852
	Brenzet Steeple			
127	Allington Knoll -	36 36 26	Bethersden Steeple	49701
	Westwell Down	68 55 44		31762
	Bethersden Steeple			
128	Allington Knoll	49 12 12	High Halden Steeple -	53827
	Westwell Down	70 39 8		44793
	High Halden Steeple			
129	Westwell Down	17 24 40	Lenham Steeple	30424
	Boughton Malherb	64 19 30		10101
	Lenham Steeple			
130	Westwell Down	12 31 21	Egerton Steeple	24722
	Boughton Malherb	30 1 45		10711
	Egerton Steeple			
131	Westwell Down	42 50 41	Turret on Romden Stables -	34586
	Boughton Malherb	71 6 34		24858
	Turret on Romden Stables			

No.	Triangles.	Observed angles.	Distances of the stations from the intersected objects.	
		° ′ ″		Feet.
132	Westwell Down -	49 12 12	Smarden Steeple {	39106
	Boughton Malherb	70 39 8		23850
	Smarden Steeple			

SECTION III.

Containing the Distances of the Objects intersected in the Survey with the small circular Instrument, from the Meridian of Greenwich, and from the Perpendicular to that Meridian. Also their Latitudes and Longitudes.

ART. 1. *Bearings and Distances*, 1795.

At Folkstone turnpike, the bearing of the station on Dover Castle in 1787, from the parallel to the meridian of Greenwich, is 65° 52′ 46″ N E (See Phil. Trans. Vol. LXXX, page 603). The new point on the Keep is 6½ feet north-eastward from the old one, which will subtend an angle at Folkstone turnpike of about 38″; therefore the new station bears 65° 52′ 8″ N E. The bearing of the centre of Tenterden Steeple from Allington Knoll, is nearly the same as that of the station in 1787, or 85° 47′ 25″ S W.: but the distances of those stations (Folkstone turnpike and Allington Knoll, see page 232 of the same Volume), from the meridian of Greenwich, and its perpendicular, are augmented in the proportion of 141747 to 141753, for obtaining the distances in the 3d and 4th columns of the following table: Folkstone turnpike being 274979 and 137920; and Allington Knoll 219935 and 144038 feet, respectively, from the meridian, and its perpendicular.

Bearings and Distances of the Stations.

Bearings from the Parallels to the Meridian of Greenwich.					Distances from merid.	Distances from perp.
	°	′	″		Feet.	Feet.
At Folkstone Turnpike.						
Dover	65	52	8	N E	303780	124318
Hawkinge	29	45	38	N E	276605	134376
At Dover.						
Paddlesworth	81	30	42	S W	262004	130553
Waldershare	36	24	53	N W	290114	105792
Ringswold	30	21	52	N E	315783	103830
At Waldersbare.						
Shore	39	54	35	N E	317545	72997
Mount Pleasant	11	56	19	N E	302716	46190
Wingham	16	36	24	N W	279533	70315
Hardres	74	21	9	N W	248180	94046
Hawkinge	25	17	53	S W		
Ringswold	85	37	43	N E		
Near the Shore.						
Ringswold	3	16	15	S W		
Mount Pleasant	28	56	58	N W		
At Mount Pleasant.						
Wingham	43	51	31	S W		
Chislet	82	23	48	S W	272918	50168
At Wingham.						
Chislet	18	10	37	N W		
Hardres	52	52	37	S W		
Beverley Park	77	3	23	N W	247060	62852
At Beverley Park.						
Hardres	2	3	23	S E		
At Allington Knoll.						
Tenterden	85	47	25	S W		
Westwell Down	32	34	49	N W	192302	100797
Wye Down	2	2	48	N E	221269	106701
Brabourn Down	22	37	5	N E	230102	119636

Interior Objects.

At Dover.						
St. Radigund's Abbey	88	5	4	N W	287597	123777
Hougham Steeple	68	19	22	S W	288341	130455
Gunston Steeple	3	43	2	N W	303189	115226

Bearings from the Parallels to the Meridian of Greenwich.					Distances from merid.	Distances from perp.
	°	′	″		Feet.	Feet.
St. Margaret's Steeple	51	54	43	N E	315148	115408
South Foreland Light-House	70	10	31	N E	315145	120721
At Waldersbare.						
Barham Windmill	61	0	4	N W	278573	93852
Elham Windmill	10	14	39	S W	284430	137246
Upper Deal Chapel	63	17	33	N E	317056	92237
Deal Castle	66	9	16	N E	321842	91768
Watch-house near the Shore	85	2	37	S E	321314	108498
Sandown Castle	55	51	56	N E	321721	84365
Walmer Steeple	73	8	30	N E	318538	97239
Ripple Steeple	70	1	50	N E	302867	97534
Waldershare Steeple	64	52	20	N E	295235	103390
Eastry Steeple	23	30	46	N E	300393	82166
Ash Steeple	4	44	29	N E	293069	70165
Minster Steeple	11	24	56	N E	301155	51113
Woard Steeple	34	11	33	N E	308967	78042
Sandwich highest Steeple	26	19	14	N E	307187	71279
Wingham Steeple	20	6	36	N W	278007	72725
Goodneston Steeple	19	16	21	N W	281915	82343
Littlebourn Steeple	27	39	59	N W	278100	70860
Canterbury Cathedral	49	51	48	N W	248198	60458
At Ringwold.						
Mongeham Steeple	21	30	34	N W	311611	93243
Norbourn Steeple	31	52	45	N W	307623	90710
Woodnesborough Steeple	29	51	29	N W	299693	75800
Near the Shore.						
Ramsgate Windmill	12	13	43	N E	321363	50817
St. Lawrence Steeple	7	30	6	N E	320817	48148
At Mount Pleasant.						
Birchington Steeple	20	17	12	N W	298729	35403
St. Nicholas Steeple	78	0	9	N W	286391	42721
Stormouth Steeple	65	26	52	S W	283143	55132
At Wingham.						
The South Reculver	8	3	15	N W	274346	33663
Hearne Windmill	34	59	11	N W	260191	42679
Blean Steeple	76	41	11	N W	241261	61259
Wickham Steeple	77	21	44	N W	270923	68384
Bridge Windmill	55	21	3	S W	260132	83723
Nackington Steeple	69	29	17	S W	249854	81418
Chillingdon Windmill	28	0	30	S E	280591	83586
Preston Steeple	5	6	13	N W	278891	63124
Shottenden Windmill	83	42	1	S W	212206	77748
Ickham Steeple	89	45	57	S W	271533	70348

Bearings from the Parallels to the Meridian of Greenwich.					Distances from merid.	Distances from perp.
	°	′	″		Feet.	Feet.
At Hardres.						
Harbledown Steeple	14	13	0	N W	241955	69535
Sturry Steeple	15	26	36	N E	256619	63499
West Stone-street Windmill	35	46	24	S W	236870	109743
Stelling Windmill	26	1	10	S W	242003	106700
On Westwell Down.						
Ashford Steeple	24	53	15	S E	200728	218961
Brook Steeple	63	10	25	S E	219234	114417
Willsborough Steeple	33	0	39	S E	206797	123109
Kingsnorth Steeple	12	49	1	S E	199037	130400
Shadoxhurst Steeple	7	20	54	S W	187830	135476
Kennington Steeple	50	34	14	S E	204869	121131
At Allington Knoll.						
Great Chart Steeple	52	21	16	N W	190572	121389
Westwell Steeple	31	46	42	N W	194208	102510
Pluckley Steeple	53	27	50	N W	173511	109641
Eastwell Steeple	25	17	49	N W	200945	103952
Charing Steeple	37	58	49	N W	182959	96677
Allington Steeple	25	0	2	N E	221344	141017
Lymne Steeple	81	23	44	S E	235914	146456
Mersham Steeple	22	32	14	N W	214269	130383
Monks-Horton Steeple	46	23	19	N E	237605	127204

ART. II. *Bearings and Distances of the Stations, and Interior Objects, intersected in 1796.*

At Goudhurst.						
Boughton Malherb	54	59	23	N E	159324	95480
Bidenden	88	49	3	N E	147431	131744
Hartridge	79	43	33	N E		
At Fairlight Down.						
Silver Hill	34	28	24	N W		
Iden Steeple	33	33	48	N E	168454	180711
Brede Steeple	13	48	32	N W	138116	197485
At Allington Knoll.						
Stone Crouch	57	3	23	S W	176642	172082
Wareborn Steeple	72	50	14	S W	193071	152324

Interior Objects.

Bearings from the Parallels to the Meridian of Greenwich.	°	′	″		Distances from merid.	Distances from perp.
					Feet.	Feet.
At Goudhurst.						
Frittenden Steeple	72	9	23	N E	135894	123079
Linton Steeple	15	32	17	N E	117510	92425
Chart Sutton Steeple	36	16	23	N E	133757	95234
Sutton Windmill	41	28	17	N E	138534	90169
Ulcomb Steeple	47	18	3	N E	147633	94491
Headcorn Windmill	57	54	53	N E	140758	111015
Staplehurst	51	49	3	N E	127216	116176
Cranbrook Steeple	71	8	27	S E	123602	138488
At Fairlight Down.						
Rolvenden Steeple	1	59	36	N E	145513	155271
Beckley Steeple	1	7	43	N E	144072	179830
Peasemarsh Steeple	25	11	9	N E	158458	186395
Whittersham Steeple	21	13	46	N E	162307	169704
Sandhurst Steeple	17	26	59	N W	127277	167613
Winchelsea Steeple	50	39	28	N E	164181	201501
Icklesham Steeple	43	12	28	N W	156031	204073
At Allington Knoll.						
Bethersden	69	11	15	N W	173469	126373
High Halden	81	47	1	N W	164672	136054
Orleston Steeple	86	50	21	S W	196655	145317
Woodchurch Steeple	89	57	22	N W	177569	144000
Warehorn Steeple	72	50	14	S W	193071	152324
Brookland Steeple	42	19	2	S W	192410	174253
Old Romney Steeple	21	3	44	S W	207322	176759
New Romney Steeple	4	41	50	S W	217098	178460
At Boughton Malherb.						
Benenden Steeple	25	12	54	S W	129542	150187
At Silver Hill.						
Brasses Windmill	40	7	4	S E	123521	187554
At High Nook.						
New Church Steeple	57	43	31	N W	214018	156687
Ivy Church Steeple	82	52	46	S W	205170	168562
St. Mary's Steeple	78	53	12	S W	204756	170287
At Ladd.						
Playden Steeple	85	1	0	N W	169333	187267

Bearings from the Parallels to the Meridian of Greenwich.		Distances from merid.	Distances from perp.
At Westwell.		Feet.	Feet.
Lenham Steeple	63 25 45 NW	165089	87178
Egerton Steeple	86 38 14 SW	167621	102243
Smarden Steeple	61 47 14 SW	157842	119273
Turret on Romden Stables	56 18 54 SW	163521	119970
At Stone Crouch.			
Appledore Steeple	30 33 49 NE	182243	162595
Snave Steeple	65 22 1 NE	200828	160993
Snargate Steeple	66 35 7 NE	193068	164969
East Guilford Steeple	6 54 4 SW	174746	187750

ART. III. *Latitudes and Longitudes of Objects intersected in 1795.*

Names of Objects.	Latitude.	Longitude east from Greenwich. In degrees.	In time.
	° ′ ″	° ′ ″	m. s.
The Belvidere in Waldershare Park	51 11 13	1 15 39	5 2.6
Ringswold, or Kingswold Steeple	51 11 8	1 22 20	5 29.3
Upper Hardres Steeple	51 13 1	1 4 45	4 19
Chislet Steeple	51 20 4	1 11 24	4 45.6
St. Radigund's Abbey	51 7 56	1 14 44	4 58.9
Hougham Steeple	51 6 50	1 15 4	5 0.3
Gunston Steeple	51 9 18	1 19 0	5 16
St. Margaret's Steeple	51 9 14	1 22 7	5 28.5
South Foreland Light-House	51 8 21	1 22 6	5 28.4
Barham Windmill	51 12 52	1 12 41	4 50.7
Elham Windmill	51 5 44	1 14 1	4 56.1
Upper Deal Chapel	51 13 2	1 22 44	5 30.9
Deal Castle	51 13 5	1 23 59	5 35.9
Watch-house near the sea shore	51 10 21	1 23 46	5 35.1
Sandown Castle	51 14 18	1 23 59	5 35.9
Walmer Steeple	51 15 29	1 23 8	5 32.5
Ripple Steeple	51 12 12	1 19 0	5 16
Waldershare Steeple	51 11 15	1 16 59	5 7.9
Eastry Steeple	51 14 44	1 18 26	5 13.7
Ash Steeple	51 16 44	1 16 34	5 6.3
Minster Steeple	51 19 50	1 18 46	5 15.1
Woard Steeple	51 15 23	1 20 41	5 22.7
Sandwich highest Steeple	51 16 30	1 20 15	5 21
Wingham Steeple	51 16 21	1 12 38	4 50.5
Goodneston Steeple	51 14 45	1 13 26	4 53.7
Littlebourn Steeple	51 16 40	1 11 1	4 44.1
Canterbury Cathedral	51 18 26	1 4 53	4 19.5

Names of objects.	Latitude.	Longitude east from Greenwich. In degrees.	In time.
	° ′ ″	° ′ ″	m. s.
Mongeham Steeple	51 12 53	1 21 18	5 25,2
Norbourn, or Northbourn Steeple	51 13 18	1 20 17	5 21,1
Woodnessborough, or Woodnesbor. Steeple	51 14 47	1 18 16	5 13,1
Ramsgate Windmill	51 19 49	1 24 4	5 36,3
St. Lawrence Steeple	51 20 16	1 23 56	5 43,7
Birchington Steeple	51 22 25	1 16 13	5 4,8
St. Nicholas Steeple	51 21 15	1 14 57	4 59,8
Stourmouth, or Stormouth Steeple	51 19 8	1 14 3	4 56,2
The South Reculver	51 22 47	1 11 50	4 47,3
Hearne Windmill	51 21 20	1 8 6	4 32,4
Blean Steeple	51 18 19	1 3 4	4 12,3
Wickham Steeple	51 17 5	1 10 48	4 43,2
Ickham Steeple	51 17 47	1 10 7	4 40,5
Bridge Windmill	51 14 35	1 7 55	4 31,7
Nackington Steeple	51 14 59	1 5 14	4 20,9
Chillingdon Windmill	51 14 30	1 14 49	4 59,3
Preston Steeple	51 17 55	1 12 54	4 51,6
Shottenden Windmill	51 15 41	0 55 25	3 41,7
Harbledown Steeple	51 16 58	1 3 13	4 12,9
Sturry Steeple	51 17 55	1 7 5	4 28,3
West-Stone-street Windmill	51 10 22	1 1 45	4 7
Stelling Windmill	51 10 51	1 3 6	4 12,4
Ashford Steeple	51 8 56	0 52 18	3 29,2
Brook Steeple	51 9 38	0 57 8	3 48,5
Willsborough Steeple	51 8 14	0 53 52	3 35,5
Kingsnorth Steeple	51 7 3	0 51 49	3 27,3
Shadoxhurst Steeple	51 6 14	0 48 53	3 15,5
Kennington Steeple	51 10 12	0 53 17	3 33,1
Great Chart Steeple	51 8 33	0 49 39	3 18,6
Westwell Steeple	51 11 39	0 50 39	3 22,6
Pluckley Steeple	51 10 30	0 45 14	3 0,9
Eastwell Steeple	51 11 23	0 52 24	3 29,6
Charing Steeple	51 12 37	0 47 44	3 10,9
Allington, or Aldington Steeple	51 5 16	0 57 36	3 50,4
Lymne Steeple	51 4 20	1 1 22	4 5,5
Mersham Steeple	51 7 1	0 55 47	3 43,1
Monks Horton Steeple	51 7 30	1 1 53	4 7,5

Latitudes and Longitudes of Objects intersected in 1796.

Names of objects.	Latitude.	Longitude east from Greenwich. In degrees.	In time.
	° ′ ″	° ′ ″	m. s.
Linton Steeple	51 13 24	0 30 40	2 2,7
Sutton Windmill	51 12 40	0 36 9	2 24,6
Chart Sutton Steeple	51 12 56	0 34 54	2 19,6
Lenham Steeple	51 14 13	0 43 0	2 52,4
Romden Stables	51 8 49	0 42 36	2 50,4
Smarden Steeple	51 8 57	0 41 8	2 44,5
Bethersden Steeple	51 7 45	0 45 10	3 0,7
Tolvenden Steeple	51 3 3	0 37 50	2 31,3
Beckley Steeple	50 59 1	0 37 24	2 29,6
Bidenden Steeple	51 7 3	0 38 23	2 33,5
Headcorn Windmill	51 10 21	0 36 41	2 26,7
Ulcomb Steeple	51 13 1	0 38 31	2 33
Staplehurst Steeple	51 9 30	0 33 9	2 12,6
Cranbrook Steeple	51 5 50	0 32 10	2 8,7
Egerton Steeple	51 11 44	0 43 43	2 54,9
Frittenden Steeple	51 8 20	0 35 24	2 21,6
Snargate Steeple	51 1 23	0 50 10	3 20,7
Snave Steeple	51 2 1	0 52 12	3 28,8
Warehorn Steeple	51 3 27	0 50 13	3 20,9
Orleston Steeple	51 4 36	0 51 10	3 24,7
Winchelsea Steeple	50 55 26	0 43 34	2 54,3
Sandhurst Steeple	51 1 3	0 33 4	2 12,3
Whittersham Steeple	51 0 39	0 42 10	2 48,7
New Church Steeple	51 2 42	0 55 38	3 42,5
Ivy Church Steeple	51 0 45	0 53 18	3 33,2
St. Mary's Steeple	51 0 29	0 53 11	3 32,7
East Guilford Steeple	50 57 50	0 45 21	3 1,4
Appledore Steeple	51 1 47	0 47 22	3 9,5
Old Romney Steeple	50 59 25	0 53 50	3 35,3
New Romney Steeple	50 59 7	0 56 22	3 45,5
Playden Steeple	50 57 46	0 43 56	2 55,7
Brookland Steeple	50 59 51	0 49 58	3 19,9
Iden Steeple	50 58 50	0 43 43	2 54,9
Brede Steeple	50 56 7	0 35 49	2 23,3
Benenden Steeple	51 3 54	0 33 41	2 14,8
Brasses Windmill	50 57 46	0 32 3	2 8,2
Icklesham Steeple	50 55 1	0 40 29	2 42
Boughton Malherb Steeple	51 12 51	0 41 34	2 46,3
Peasemarsh Steeple	50 57 54	0 41 7	2 44,5
Woodchurch Steeple	51 4 51	0 46 12	3 4,8
High Halden Steeple	51 0 11	0 42 52	2 51,5

CONCLUSION.

THE account contained in the foregoing pages is presented in its present form, agreeable to the resolution expressed in our last communication. It is there stated, or rather implied, that, as materials are collected, details will meet the public eye through the medium of the Philosophical Transactions. The publishing of these particulars at periods not very remote from each other, will prove convenient, as we shall be enabled to communicate many *data*, which would be necessarily withheld, were these disclosures less frequent. It is on this account, that the particulars in *Part the First* do not contain the latitudes and longitudes of the stations, and objects intersected, as sufficient *data* have not yet been obtained for making the computations in an unexceptionable manner: but the contents of the *Second Part* are more complete, that Survey having been carried on in a country sufficiently near the meridian of Greenwich to give the necessary arguments with precision.

It is perhaps scarcely necessary to observe, that the design intended to be answered by an admission of the plans of the triangles annexed to this account, is to enable the reader to comprehend with ease the state of the operation, and to apply, without difficulty, the materials found in the body of the work to future Surveys. We have therefore, not attempted to delineate any varieties of ground in the plan of the western triangles (Tab. XI.): and it may, in this place, be proper to mention,

that the ranges of hills expressed in the plan found in our last account, were copied from authorities of the late Major General Roy. The map now given, of the operations performed in Kent (Tab. XII.), has the ground depicted in as accurate a manner as the scale will admit of, Mr. Gardner, from the minuteness of this Survey, being enabled to do it with accuracy.

On adverting to a principal object of this undertaking, that of preparing materials for correcting the geography of the country, it may be expected something should be said, respecting the accuracy of the maps of those counties in which our operations have been carried on. It is almost unnecessary to observe, that great correctness cannot result from the methods commonly taken in large surveys, which are usually made with an apparatus altogether unfit for measuring angles or bases with a sufficient degree of accuracy: and it will evidently appear, on applying the distances given in this, and our former paper, to those maps, that they are, generally, very defective. We must, however, observe, that Linley's and Crossley's Map of Surry, and Gardner's Map of Sussex, are the best which have yet fallen under our notice: the first is, in some measure, indebted for its excellence to the Trigonometrical Operation in 1787; and the latter to our own; as the distances between many stations, and the situations of many churches, in the southern, and western parts of Sussex, were given to Mr. Gardner prior to the publication of our last account. The geography of Devonshire and Dorsetshire is found particularly erroneous, as may be easily discovered by an application of our distances to the best maps of those counties.

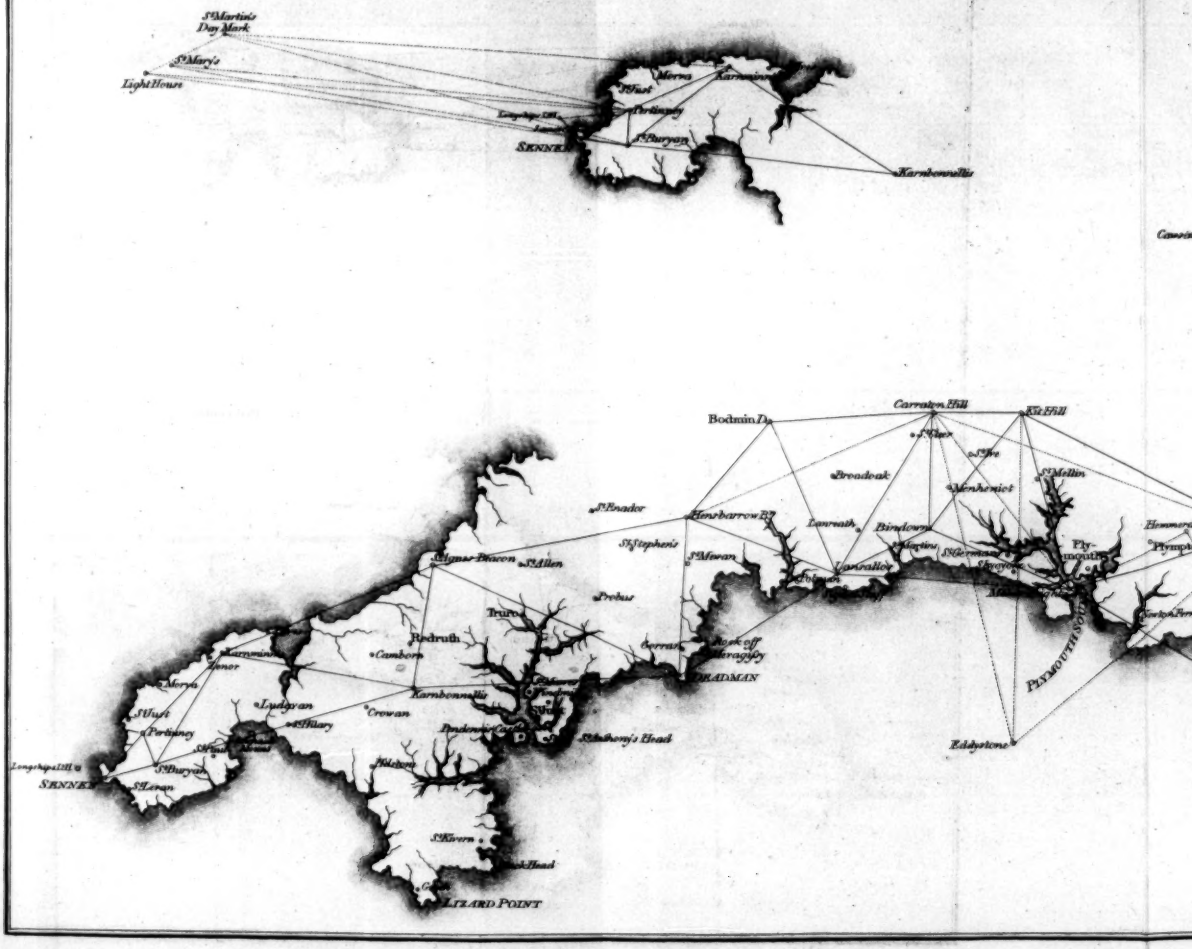
N. B. In Tab. XI. the triangles connecting the three principal objects in the Scilly Isles, and the stations from whence they were intersected, are laid down in that detached position to shorten the plan.

Errata in the Account of the Survey, *Phil. Trans.* 1795.

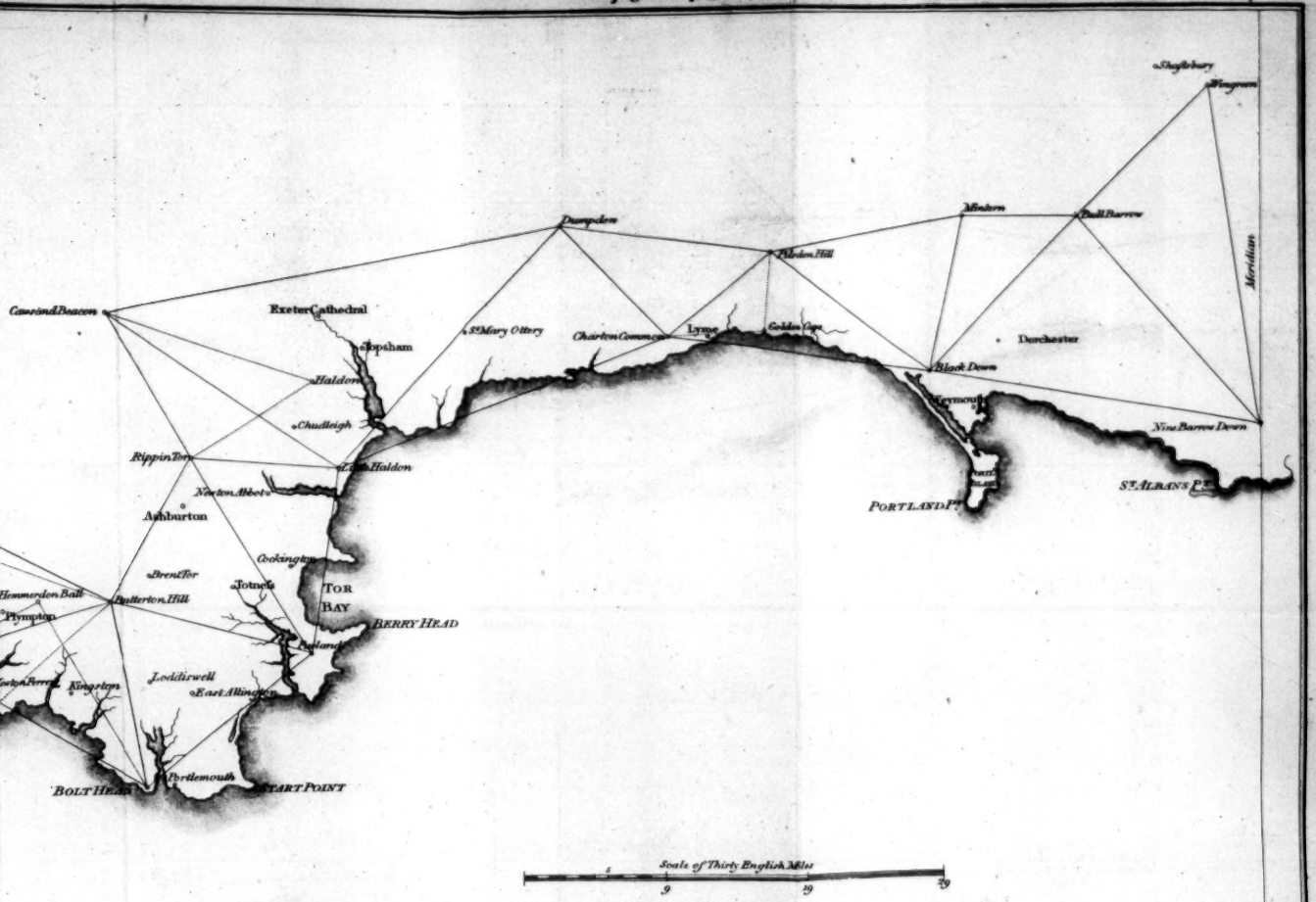
Page 469, line 4, *for* 124 *read* 125.
507, line 18, *for* 258 *read* 285.
527, in the table, *for* 51° and 60° *read* 50° and 66°.
ib. *ib.* col. 4, *for* 30 *read* 33.
554, against Southwick Church, *for* 57710 *read* 5771.
558 *et alibi*, *for* Mitford *read* Milford.
559 *et alibi*, *for* Funtingdon *read* Fordington.
580, line 10 from bottom, *for* 39′ *read* 47′.
584, lines 2 and 3, *for* $\frac{1}{15}$ *read* $\frac{1}{13}$.

The triangles numbered 84, 100, 105, *are doubtful, and consequently the results depending on them are uncertain.*

PLAN *of the* PRINCIPAL TRIANGLES *in the*

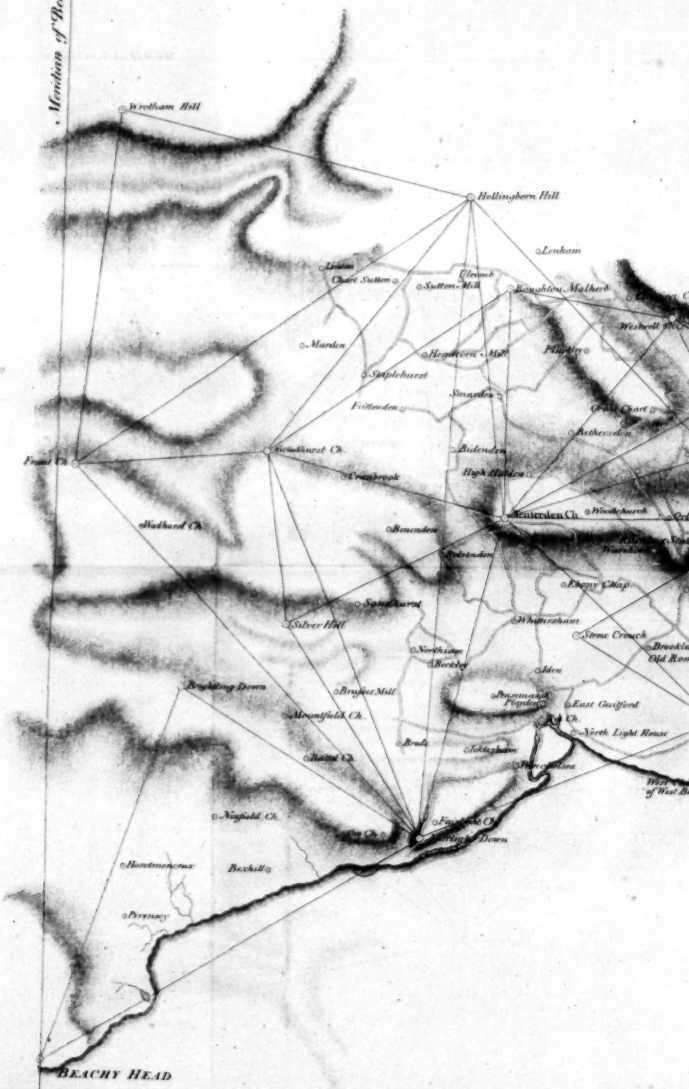


Philos. Trans. MDCCXCVII. Tab. XI. p. 542.



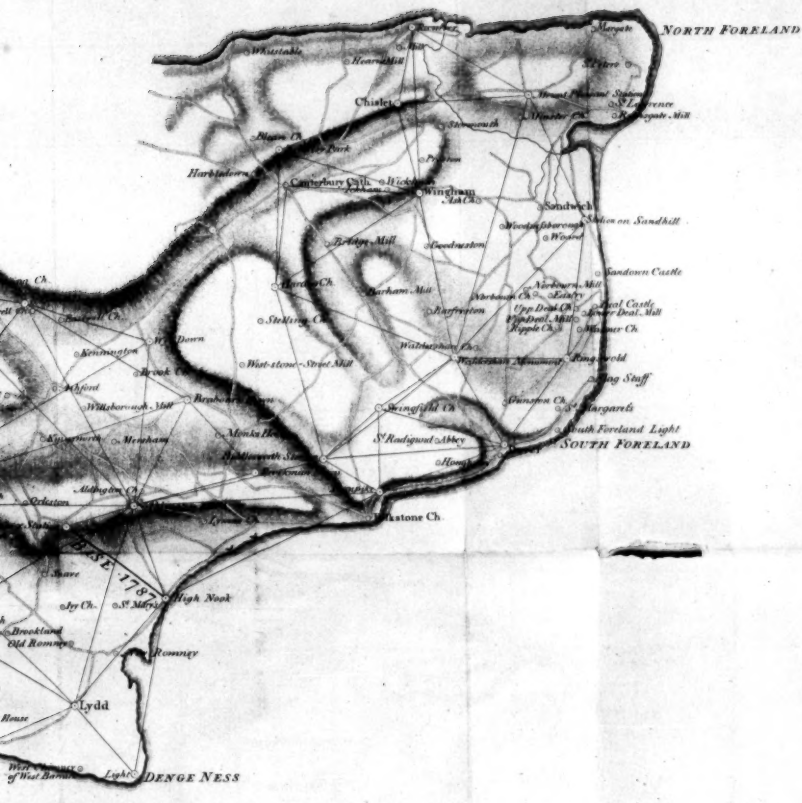
Beaufoy Sc.

Part of KENT and SUSSEX laid down from



from a **TRIGONOMETRICAL SURVEY** *in 1795–1796.*

Philos. Trans. MDCCXCVII. *Tab.* XII. *p.* 549.



Scale of Twenty English Miles

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ERRATA.

Page 259. line 5 below the table, *for* 436800 *read* 412529.
386, at end of paragraph, *after* lithic *add* acid.
393. line 2, *for* earth *read* earths.
397. line 17, *for* By acid of *read* By aid of.

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